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ON THE DEVELOPMENT OF EARLY CONDITIONED REFLEXES AND DIFFERENTIATIONS OF AUDITORY STIMULI IN INFANTS

BY N. I. KASATKIN AND A. M. LEVIKOVA[1]

Pedological and Neurological Infancy Clinics and the Developmental Department of the Bekhterev Brain-Institute

1. INTRODUCTION

One of the most important points in the development of a child is reached at the moment when the highest sections of the cerebrum attain the ability to form conditioned reflexes, *i.e.* the ability to form connections between the phenomena of the outer world. The problem of the exact time when such early connections may be formed has been differently solved by different investigators of infancy—psychologists as well as physiologists. Canestrini (1) states that the infant from its first days distinguishes its mother from other people, reacting to her stimuli (whispering) in a different way than to sounds originated by strangers. In other words, Canestrini believes that in an infant of a few days there is already a complex differentiating of relations to the outer world. Giving due credit to Canestrini who has greatly contributed to the study of infancy by introducing exact physiological methods, we must however emphasize the convincing criticism of Volkelt (2) and Peiper (3) concerning the methodology of Canestrini's

[1] The manuscript has been read and corrections have been made by Professor Margaret Wooster Curti of Smith College.

experiments and therefore also concerning his conclusions. Preyer (4) after observing the development of his own child tells us of his impression that it "formed a connection between the sounds heard (song of his mother) and its mother's face" only in its sixth week.

Only an experimental study of the child enables the investigator to give exhaustive material which would help him to solve this problem. The method of conditioned reflexes, used by contemporary natural science, seems to be quite satisfactory from the point of view of exact science, being the only objective one. This method has been brilliantly developed by Pavlov (5) and his school as well as by Bekhterev and his collaborators (6). The study of the conditioned reflexes in children was first attempted by a pupil of Pavlov, Krasnogorsky (7). He, as well as Peiper and Helmreich (8), who uses Krasnogorsky's data, reported that their subjects during the first two to six months of life did not form any conditioned reflexes and, therefore, this observation is considered evidence of a lack of functional activity of the cortex during those months. According to Krasnogorsky, however, it is possible that the first conditioned reflexes may be formed during the fourth or fifth month of life. In contradiction to this Bekhterev and Shchelovanov (9) have pointed out the possible formation of some of the naturally arising conditioned reflexes as early as the end of the first month of the infant's life.

2. Methods

The opening of the infant's mouth was considered as an index of the conditioned reflex by Krasnogorsky in his study of conditioned reflexes. He and Lashley simultaneously used a special salivary capsule for the registration of the conditioned salivary reaction of the infant. Krasnogorsky succeeded in getting this conditioned reaction in contradiction to the experiments of Richter and Wada (10). The application of the salivary method during the first three months, however, seems to be rather difficult in view of the extremely poor activity of the salivary glands. On the other hand the reaction of the opening of the mouth as a conditioned reaction in response to an alimentary stimulus, according to our data, arises only during the third month of the infant's life and sometimes even very much later.

A special method making use of the sucking movements as a means of demonstrating the presence or absence of the conditionary reflex has been developed under the direction of Shchelovanov by the Developmental Department of the Bekhterev Brain-Institute and by the Pedological and Neurological Infancy Clinics. The first variations

of this method were applied by Denisova and Figurin (11) and by Levikova and Nevmyvak (12) [2] of these institutions.

The expressiveness of the reactions of infants in the first two months of their lives to any touch within the region of the mouth is a well known fact. Vigorous sucking movements start at the moment when the lips of the infant get in touch with the mother's breast in the feeding position or at the sight of the breast or the feeding bottle and the like. This is the natural conditioned reflex. Experiments in this laboratory show that, should a stimulus indifferent to the infant (sound, light or touch) be introduced regularly under the conditions of the experiment, some seconds before feeding from the feeding bottle or at the breast of the mother, vigorous sucking movements arise after a certain time at a certain age. They occur at the moment when the action of the chosen stimulus begins. The principal condition for the formation of the conditioned reflex is the coincidence in time of the conditioned stimulus with the non-conditioned one.

In our present investigation we used as a conditioned stimulus the sound Mi—2 of an organ pipe (produced by a gasometer) and as a non-conditioned stimulus a feeding bottle containing from 80–100 cc. of milk. The experiments were carried out once a day at the usual feeding time in order to obtain an equal alimentary excitement.

The infants to be studied were taken to the laboratory in turn and placed in an experimental crib, furnished with a special hood protecting the child from any outside influences. The hood was fitted with a series of different devices. The eyes of the infant were covered with a bandage. The sucking movements and breathing during the experiment were recorded by a kymograph. A hollow rubber ball was put under the chin of the infant and connected by means of a pneumatic transmission with a Marey recording drum which served for checking of the sucking movements.

During the experiment the infant was from 6 to 12 times fed with milk from the feeding bottle and each feeding was preceded by a sound of the tone Mi—2. The interval between the sound and the touch of the feeding bottle on the lips of the infant in the first experiment was 2 to 3 seconds; after the stabilization of the conditioned reflex this interval was extended to 10 to 15 seconds, while the feeding itself lasted from 15 to 30 seconds. The intervals between separate combinations varied from 10 seconds to 1 minute depending on the degree of excitement of the infant and also in order to avoid the formation of a conditioned reaction to time rhythm. The whole experiment lasted from 10 to 15 minutes each day.

During the whole course of the experimental work each child underwent from 70 to 120 such experimental procedures in the course of 4 or 5 months. At first when the conditioned reflexes had not yet been formed, the sucking movements appeared at the moment when the infant's lips came into contact with the feeding bottle. After its formation, they occurred immediately on the application of the conditioned stimulus (the tone Mi—2).

The impossibility of getting the salivary reflex during the first three months of the infant's life, as well as the absence of the genetically later reaction of the opening of the mouth ought to be taken into consideration. That is why the described sucking movements are the best, if not the sole convenient reaction, that enables us to study the conditioned alimentary reflexes of the infant in the first few months of its life.

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[2] Levikova and Nevmyvak conducting their study under laboratory conditions had obtained a combined reflex to auditory stimuli in infants at the age of 2 months and 6 days to 3 months and 20 days. According to the authors, the late formation of combined connections in their study might be a consequence of interruptions occurring during the experimental work.

3. Subjects

Three children, aged 11 days to 1 month (at the beginning of the experimental work) had been chosen as the objects of our study. They all came from the Hospital for Normal Children of the Pedological and Neurological Infancy Clinic. They all had a normal heredity and were well cared for and fed by their mothers, factors promising a normal development.

4. Description of the Experiments

A. The Formation of the Alimentary Conditioned Reflex.—Passing to the description of our basic experimental data, we have first to note the general type of development of the conditioned reactions in all our subjects. The records quoted below characterizing the various stages in the development of the conditioned reflex in one infant are characteristic of all of the rest.

Our first experiments, not illustrated in the table, showed no reaction on the part of the infant to the sound Mi—2 chosen as a conditioned stimulus. When applied 2 or 3 seconds before feeding, this sound has until now remained an indifferent agent and this infant, like all the infants, started its sucking movements only at the contact of its lips to the feeding bottle.

Experiment No. 2 (October 12—*cf.* Table I) shows no changes in the reactions of the infant to the sound of the organ pipe, the reaction remaining the same as before. If the infant was whining it went on whining; if calm, it remained calm. If at intervals between the application of the combined stimuli spontaneous sucking movements occurred, they did not change in vigor or intensity.

The period of indifference on the part of the infant with regard to the conditioned stimulus may be considered as the first stage in the process of the formation of a conditioned reflex. Towards the end of the first and the beginning of the second month of life, a slight tendency to react to the tone alone seemed to appear. First it was possible to notice clearly a general inhibition of movements. The infant who was whimpering, moving uneasily or even crying at the beginning of the sound either lowered its crying or whining or even entirely stopped. The movements of the limbs also became slower or stopped entirely (*cf.* Table I, Exp. No. 6, October 22).

TABLE I

SELECTED GROUPS OF TRIALS SHOWING STAGES ON CONDITIONING IN THE CASE OF ONE INFANT, R., BORN SEPTEMBER 11

In all records the number of a combination when unaccompanied by the sign of the tone means the basic combined signal,—the tone Mi—2.

Number of the Combination	Duration of Non-Conditioned Stimulus in Seconds	Isol. Action of Conditioned Stimulus (Tone) in Seconds	Conditioned Reflex	Reaction to the Conditioned Stimulus (Tone)	Support	Interval between Trials, in Seconds	Behavior in the Intervals
				Experiment No. 2, October 12. I, I (Subject age 1 mo. 1 day)			
6	10	2	−	Calm	+	15	Calm, moving, spont. sucking mov.
7	15	2	−	Calm	+	20	Calm moving
8	10	2	−	Calm	+	30	Calm
9	10	2	−	Calm	+	15	Calm whimpering
10	10	2	−	Calm	+		Calm
				Experiment No. 6, October 22, I, II (Subject aged 1 mo. 11 days)			
24	20	3	−	Whimpering	+	15	Moving
25	20	3	−	Moving	+	10	Moving, rare, spont. sucking mov.
26	20	3	(+)	Immediate inhibition	+	15	Moving
27	15	3	(+)	Immediate inhibition	+	15	Moving weak, spont. sucking mov.
28	15	3	+?	Im. inhib. & suck. movements (?)	+	20	Moving rare, spont. sucking mov.
29	15	5	+?	Sucking movements (?)	+		
				Experiment No. 7, October 24, I, 13 (Subject aged 1 mo. 13 days)			
30	15	5	+	Inhibition & clear sucking mov.	+	20	Whimpering
31	15	5	+	Inhibition & clear sucking mov.	+	15	Moving rare, spont. sucking mov.
32	15	8	+	Inhibition, clear sucking mov.	+	20	Same behavior
33	15	5	+	Inhibition, clear sucking mov.	+		

Slight movements of the infant's mouth began almost simultaneously. At the isolated sound of the tone Mi—2 in two or three combinations during one experiment, after a certain slowing down of the movements, there arose slight movements of the mouth or lips, or at intervals, spontaneous

sucking movements if already present became more vigorous and frequent. The condition of a growing inhibition of movements at the application of the conditioned stimulus and a vague conditioned reflex may be considered as the second stage—a transitory step towards a firmly established and clearly expressed conditioned reflex.

Obvious combined sucking movements beginning at once at the sound of the auditory signal and continuing strongly and rhythmically until the insertion of the feeding bottle in the infant's mouth mark the third and last stage of the process of the formation of the conditioned reflex. This last stage is illustrated by Exp. No. 7, October 24 in Table I and by the kymograph record, which is not reproduced here. This is the time when in this particular infant all conditioned reflexes consisted of obvious sucking movements. Thus clear conditioned reflexes were present at the age of 1 month and 13 days.

In some infants the conditioned response appeared somewhat earlier, as is shown in Table II below. Though of

TABLE II

There are intervals of 2–3 days during the experimental work. Had the work been continuous the reflex would probably have been formed somewhat earlier by the first infant (R).

Number	Initial	Age at Beginning of Experimental Work	Date of Birth	Number of Experiments	Number of Combinations	Age at the Moment of the Formation of Conditioned Reflexes	Date of Appearance of Conditioned Reflexes
1	R	1 mo. 1 day	Sept. 11	6	29	1 mo. 13 days	Oct. 24
2	P	25 days	Oct. 22	12	74	1 mo. 9 days	Dec. 1
3	E	11 days	Oct. 19	18	131	1 mo. 4 days	Nov. 24

different ages at the beginning of our work and though passing through a different number of confirmative experiments, the infants formed the conditioned reflexes at about the same age. *This leads to the conclusion that before a certain age no conditioned connections can be formed in an infant's central nervous system—i.e. their formation depends on the degree of maturity.* While the oldest of the infants under experiment was exactly

1 month old at the beginning of the experimental work and only 29 confirmative experiments were necessary in his case to create a firm conditioned connection, the youngest infant who was only 11 days old required for his part 131 repetitions to create a conditioned reflex of a similar degree of firmness.

B. The Formation of the Sound Differentiation.—Concerning the creation of auditory differentiations we may admit *a priori* that besides the initial stimulus, which is answered by an already established conditioned reflex, there is a number of similar agents which, by means of associative processes, are able to call forth the same conditioned reaction as the initial conditioned stimulus. On the other hand there is a number of other agents which, being rather remote in the physiological sense, will have no effect, *i.e.* will be at once differentiated.

Acting on this supposition we introduced a new stimulus as a differentiated sound, one never before accompanied by alimentary support, which consisted of the sound of a bell entirely different from the sound Mi—2 used as a conditioned stimulus. The reactions of the infants to the sound of the bell when used for the first time were rather different and we shall therefore give a separate description for each infant tested.

1. *Infant T. R.*—The bell was rung 44 times. The first records show that the sound of the bell, though not accompanied by feeding, called forth the same response as the conditioned stimulus, that is, inhibition and intense sucking movements which were in some cases superseded by crying.

Evidently we have here an association of the newly introduced differentiated stimulus—the ringing of the bell—with the conditioned stimulus (Mi—2). The further process was entirely similar to that of the development of a new differentiation. In accordance with this we see definite stages in the gradual change of the response to the bell. The first stage, as we have already seen, shows a complete association expressed by an alimentary reaction appearing at the sound of the bell. In other words, there is no differentiation at all. While continuing the use of the bell as a stimulus without giving alimentary support, we note, in the next few experiments already, a decrease of the sucking movements immediately after the bell has been sounded.

The following second stage shows only 1 to 3 sucking movements mostly after some delay and showing strikingly less vigor than the sucking movements in response to the initial conditioned tone Mi—2. Finally a few experiments later, the alimentary reflex to the ringing of the bell is entirely absent—hence we can conclude that there has been a complete differentiation characteristic of the third stage of the reflex activities. By that time the infant was 2 months and 22 days old.

2. *Infant G. K.*—In our experiments with this child the bell was rung 54 times. As a rule there was no alimentary reaction to the bell, whereas the conditioned reflex to the initial tone Mi—2 remained in full vigor. Yet the bell was not always an inactive stimulus. With regard to this infant in three cases a clear conditioned reaction to it like that to the tone Mi—2 could be noticed. The moment of the formation of these combined reactions to the bell in this infant has no regularity whatsoever; they arise suddenly after a number of experiments showed no alimentary reaction to the sound. This leads us to the supposition that the association of the bell with the conditioned stimulus could happen only in the experiments during which the alimentay excitability of the infant was somewhat higher than usual.

Finally we must note one more feature typical of the behavior of the infant at the sound of the bell. Very often, as soon as the bell began to ring or a few seconds later, the child which had been crying or moving uneasily showed an inhibition of all general movements and after some period of relative quiescence once more changed to crying. In some cases after a short period of inhibition the infant answered the sound of the bell with crying. Later we shall return to the analysis of this form of reaction.

3. *Infant L. E.*—This infant was stimulated by the bell 22 times. We note a distinct conditioned reflex as well as the absence of alimentary reaction to the bell, which would have proved the association of the bell with the conditioned stimulus. Hence in this case it would seem that a differentiation was obtained at once.

Thus the infant T. R. at first showed no differentiations to the sound of the bell, but the differentiation began and was completely developed during the second half of the third month of the infant's life,[3] while in the cases of G. P. and L. E. the sound of the bell was differentiated at once and no conditioned reflex appeared. It is of interest to note this individual variation to the range of generalisation in different infants and the mobility of even such a rough differentiation.

C. The Formation of the Tone Differentiation.—The study of the effect of the bell was started at the same time as the development of the differentiation of tone Fa of the minor octave differing from the initial conditioned tone by almost two octaves (11½ tones). This newly introduced tone Fa is a stimulus of the same kind as the initial sound and is nearer to it than the bell.

The new stimulus was at first used 1 or 2 times during one experiment. The process of the development of the differentiation began, as might be expected, with an association of the new tone with the initial one. When sounding for the first time and not supported by milk-feeding from the feeding bottle, the newly introduced tone called forth a series of distinct rhythmical sucking movements. After stating these general facts we may pass to the description of the process of the development of the differentiation in the different subjects.

1. *Infant T. R.*—The development of the differentiation of the tone Fa (minor octave) began at the age of 2 months and 14 days. The infant showed a clearly expressed sucking reflex and had a well established conditioned reflex to the initial tone.

During the first experiments the reaction to the tone Fa was the same as to the conditioned stimulus tone Mi—2, *i.e.* the infant showed inhibition of general movements at once, and distinct vigorous sucking movements began that continued either the whole time of the phonation of the tone Fa or only during the first 7 to 10 seconds, when they were interrupted by crying, after which they were resumed and went on

[3] Taking an interest in the absence of any immediate differentiation of the bell, showed by the infant T. R., we several times used as a new stimulus a rather low knocking not accompanied by any alimentary support. In all cases we could not note the slightest alimentary reflex.

until the end of the experiment (*cf.* Exp. 40, Dec. 5 in Table III). During that period not only the usual conditioned reflex was as vivid as before, but an associated combination which followed the unsupported differentiated stimulus some 15 to 20 seconds later gave a complete conditioned effect.

After a certain time, in Exp. No. 44, Dec. 10 (Table III) at the 12th application of the new stimulus (fa-12), we note a difference in the infant's reaction to the conditioned and the differ-

TABLE III

TONE DIFFERENTIATION IN INFANT T. R.

No. of the Combined Stimulation	Action of Non-Conditioned Stimulus (Seconds)	Isolated Action of Conditioned Stimulus, Tone Mi-2 (Seconds)	Conditioned Reflex	Reaction	Support	Interval	Behavior in the Intervals
				Exp. 40, Dec. 5, Age, 2 mo. 25 days			
241	15″	5″	+	Immed. vivid sucking movements	+	8″	Whines
fa—7		15″	+	Immed. vivid suck. mov. lasting whole time	−	15″	Whines
242	15″	5″	+	Immed. vivid suck. mov.	+	20″	Whines
fa—8		15″	+	Immed. vivid sucking mov. 8 sec.; whining; then again vivid suck. movements	−	15″	Whines
243	15″	5″	+	Immed. vivid sucking mov.	+		Whines
				Exp. 44, Dec. 10, Age, 3 mo.			
259	15″	10″	+	Immed. vivid sucking mov.	+	20″	Whines
fa—12		15″	+	Vague suck. mov. 5 sec.; vigorous suck. mov. 3 sec.; rare suck. mov. and whining	−	10″	Whines
260	15″	10″	+	Immed. vivid sucking mov.	+		
				Exp. 46, Dec. 13, Age, 3 mo. 3 days			
276	15″	7″	+	Immed. vivid sucking mov.	+	15″	Whines
fa—15		20″	−	Immed. inhibition lasting whole time	−	7″	Whines
277	15″	5″	+	Immed. vivid sucking mov.	+	15″	Whines
fa—16		20″	−	Moves during 1 sec., then complete inhibition whole time	−	10″	Whines
278	15″	5″	+	Immed. vivid sucking mov.	+		
				Exp. 47, Dec. 14, Age, 3 mo. 4 days			
262	15″	5″	+	Immed. vivid sucking movements	+	25″	Whines
fa—17		20″	+	Immed. vivid suck. mov. 7 sec.; whining; again suck. movements	−	10″	Whines

TABLE III—*Continued*

No. of the Combined Stimulation	Action of Non-Conditioned Stimulus (Seconds)	Isolated Action of Conditioned Stimulus, Tone Mi-2 (Seconds)	Conditioned Reflex	Reaction	Support	Interval	Behavior in the Intervals
283	20″	5″	+	Immed. vivid sucking mov.	+	15″	Whines
284	15″	5″	+	Immed. vivid sucking movements	+	20″	Whines
fa—18		25″	+	Immed. suck. mov. 8 sec.; crying 2 sec.; suck. mov. whole time	−	15″	Whines
285	20″	7″	+	Immed. vivid sucking mov.	+		
304	10″	11″	+	Suck. mov. after the lapse of 8 sec.	+		
				Exp. 54, Dec. 24, Age, 3 mo. 14 days			
332	15″	5″	+	Immed. sucking movements	+	15″	Calm
fa—30		30″	−	Inhibited, calm, moves	−	25″	Calm
335	15″	5″	+	Immed. sucking movements	+	25″	Calm
fa—31		30″	−	Immed. inhibition continues whole time	−	10″	Calm
336	10″	5″	−	Calm	+		
				Exp. 61, Jan. 4, Age, 3 mo. 25 days			
381	15″	5″	+	Immed. inhibition and vivid sucking movements	+	20″	Cries
fa—46		20″	−	Immed. inhibition 1 sec., then violent crying	−	5″	Cries
382	15″	3″	+	Immed. inhibition and vivid sucking movements	+	20″	Cries
384	15″	5″	+	Immed. inhibition and vivid sucking movements	+	20″	Cries
fa—47		15″	−	Immed. inhibition 1 sec. and sharp crying	−	5″	Cries
385	15″	4″	+	Immed. inhibition and vivid sucking movements	+		
				Exp. 65, Jan. 9, Age, 4 mo.			
412	20″	5″	+	Immed. vivid sucking mov.	−	15″	Calm, whines
fa—55		25″	−	Calm	−	5″	Calm
413	20″	5″	+	Immed. vivid suck. movements			

entiated tone. The combined reaction to the tone Mi—2 is fully maintained whereas the reaction to the differentiated tone Fa (minor octave), which is not supported by feeding, becomes much weaker. Further in Exp. 46, Dec. 13 (Table III) we do not see any conditioned reaction to the stimulus differentiated; the infant at once ceased to move and only towards the end of the phonation of the tone Fa moved slightly again.

Yet in the following Exp. No. 47 of Dec. 14, No. 48 of Dec. 15 and No. 49 of Dec. 18, vivid sucking movements pointing to the absence of differentiation appear again in response to the differentiated stimulus. Only in later experiments, No. 50 of Dec. 19 and No. 51 of Dec. 20 (not reported in Table III), we see no alimentary reaction to the tone differentiated and a nearly complete inhibition of the general movements of the infant while this tone is sounded. Nevertheless, Exp. 52 of Dec. 21 (not in table) once more shows the absence of differentiation, which seemed to have been definitely formed. At last Exp. 54 of Dec. 24 at the age of 3 months and 14 days (Table III) and all of the following experiments testify to the presence of a firmly and definitely established differentiation.

It is interesting to note in the last Exp. No. 54 (Table III) an after-effect of the tone differentiated, the diminution or even entire absence of the conditioned effect to the tone Mi—2 when following the former combination (No. 336).

A general survey of the described period in the development of the differentiation (Dec. 13–Dec. 24) enables us to note the considerable instability of the differentiations formed, which seems to be the most characteristic feature of this period. Records in which a case of differentiation is indicated alternate with those showing absence of differentiation. Then in its further development the differentiation gradually attains a stage of firm stabilization. Nevertheless we must note that even after having been firmly established, differentiation may sometimes show greater or lesser vacillation.

After the tone Fa had been differentiated we continued to use it in our further experiments and easily noticed a change in the reactions of the infant. In the first experiments after the formation of the differentiation the most typical feature was a general inhibition of movements from the very moment of the application of the stimulus differentiated. The infant lay absolutely quietly, whining slightly from time to time, or moving its lips at rare intervals in a scarcely noticeable way. This continued during several experiments. Then a new reaction appeared. The infant who had been whining or crying feebly at intervals now instantaneously became inhibited for a

moment at the sound of the tone Fa, then suddenly redoubled its crying which continued as long as the tone sounded. In some experiments this increased crying was extremely demonstrative. (*Cf.* Table III, Exp. 61, Jan. 4.)

Figurin and Denisova (**11**) noting an inverse reaction in their experiments consider that in that case a new conditioned reaction arises, hence there is no inhibition at the time of differentiation.

Finally there is the last form of reaction—an indifferent attitude towards the stimulus, appearing more or less at the same time as the reactions described above. The behavior of the infant while the tone sounds is the same as in the interval preceding the sound (*cf.* Table III, Exp. 65, Jan. 9). These last two types of reaction close the process of development of the differentiation of tone Fa (Differentiation No. 1).

Having demonstrated the distinct differentiation of two sounds presenting an interval of 11½ tones by the infant of 3 months and 14 days, we naturally become interested in the possibility of the development of more subtle auditory discrimination. We used for the first time in Exp. No. 65, when the child was 4 months old, the tone Fa—1 (Differentiation No. 2) without accompanying this new tone with an alimentary support, the customary procedure when introducing a new stimulus to which it is desired to develop a differential response. In this experiment (latter part of Exp. 65 which is not shown in the table) there is a clear absence of differentiation. The infant associated the new stimulus—tone Fa—1, with the initial conditioned tone Mi—2. The same is true for Exp. No. 66 and 67, also not reported in the table.

In Exp. 68 and 69 the tone Fa—1 was not used, whereas the former already completely differentiated tone Fa (Differentiation No. 1) was sounded repeatedly. When tone Fa—1 (Differentiation No. 2) was used once more in Exp. 70 of Jan. 15, an abrupt change in the reaction to this tone was noted at once. Instead of the association which would mean the absence of differentiation we see a typical case of differentiation. The obtained differentiation No. 2 maintains itself in all further experiments. It is impossible clearly to describe

its development because of the irregularity of its rise. The material described in connection with differentiation No. 2 shows with full certainty that *an infant of 4 months and 4 days (Exp. 70) is able to differentiate as small a difference as 5½ tones.*

We tried to get a still more subtle differentiation and used a new tone—tone La—1 (Differentiation No. 3)—after making certain that the preceding differentiations were firmly established. Nevertheless the degree of stability of the differentiated tone La—1 just described greatly differed from that of the formerly used tone Fa—1, notwithstanding their proximity by an interval of only 2 tones. The records clearly show that Differentiation No. 2 (5¼tones), once formed, was always maintained showing only slight vacillations, whereas Differentiation No. 3 is marked by its unsteadiness. This is especially evident whenever the general condition of the infant changed. Thus a heightened excitability during the experiment, expressed by loud crying in the intervals between the combinations, destroys the stability of the differentiation. Listlessness and weakening of reactions in general also prevent the differentiation from being precise by blurring the differences between the reactions to the combined and the differentiated signals.

Besides this easily defined influence of the general state of the infant on the degree of clearness of the last differentiation obtained, the complex of the differentiating reaction will often vary during a single experiment. Thus Differentiation No. 3 of tone La—1 obtained in some experiments shows extreme instability. Though it seems to have been formed at once it often disappears in further experiments, being dependent on the general condition of the subject and in spite of being used during the whole course of the experiments it did not give a clear idea of the process of differentiation. In contradiction to the previous differentiations No. 1 and No. 2, Differentiation No. 3 cannot be described as a distinctly formed one. It may be supposed that this exactness of differentiation (3½ tones) presents a great difficulty, possibly even is the limit for the infant of the age described.

Everything described so far on the differentiation applies also to our attempt at getting a finer differentiation of tone Si—1 toward the end of our experiment. This sound differs from the initial conditioned signal by 2½ tones only. It could be foreseen how great a difficulty the differentiation of these two stimuli would present to the infant.

Still, Differentiation No. 4 as well as No. 3 was obtained at once. In their further development the infant's reaction to the last of the differentiated tones (Si—1) during the whole time while it was sounded were similar to its reactions to the tone La—1. Sudden lapses followed by faint renewals of the differentiation appeared in the course of the experiment. Then we see a sudden complete differentiation, followed by its absence. The last obtained differentiation shows an especially strong dependence on the general condition of the infant as well as on the usual type of its conditioned reactions. All these facts, while allowing us to note the periodical appearance of differentiations, at the same time bring strongly in evidence its extraordinary instability during that period of the infant's life.

2. *Infant G. P.*—The formation of Differentiation No. 1 in response to tone Fa was started when this infant was 1 month and 2 days old. At the beginning of our experimental work we observed the absence of any kind of differentiation. Exp. No. 22 of Nov. 26, shows a clear alimentary reaction to the differentiated stimulus (not given in table). This alimentary reaction of the infant was less expressive than that of the first infant in this experiment. Very often in the earlier experiments with G. P., the combined reflex manifested itself by 2 or 3 sucking movements, later on by half opening of the mouth. This peculiarity can evidently be explained by a weaker alimentary excitability or by a less pronounced "voracity" of this subject.

In spite of these individual peculiarities of the alimentary reactions of this infant, Exp. 44 of Dec. 21 shows weaker sucking movements in response to a nonsupported differentiated tone in comparison with the reaction to the combined stimulus. The form of a constant discrimination of the tones usu-

ally appears after a set of different variations. Hence when referring to the age of 2 months and 10 days, all further experiments show the maintainance of the differentiation. But as it has already been mentioned even a firmly established differentiation shows some fluctuations in the degree of its expressiveness.

A survey of the process of the development of differentiation by this second infant leads us to the conclusion that in this case a period of instability (Dec. 21–Jan. 2) spreading over 7 experiments, must be admitted. By the end of the third month of the infant's life the combined alimentary reaction (response to the conditioned stimulus) grew weaker and even entirely disappeared in some of our experiments.

Having ascertained the impossibility of continuing our observations on the further development of differentiations, we for some time gave an alimentary support to the infant as soon as the combined tone sounded. At the sound of the differentiated tone, an empty milk bottle was inserted into the infant's mouth. Having repeated this procedure several times we resumed our usual experiments. Now the isolated sound of the conditioned tone was followed not by sucking movements as before but by a wide opening of the mouth which expressed the conditioned reflex. Thus the effective expression of the conditioned reflex has undergone a change. At the same time, the already observed differentiation disappeared: the infant responded to the differentiated signal by opening its mouth. Continuing our experiments we succeeded in restoring the former differentiation expressed by a general inhibition of the infant at the sound of the differentiated tone. But, being excited in several cases, the infant answered the differentiated stimulus by increased crying. But the differentiation gradually disappeared again, thus obliging us to drop our work with this subject.

3. *Infant L. E.*—The first of the differentiated stimuli—tone Fa—was sounded at the age of 1 month and 11 days in Exp. No. 14. The infant responded to the nonalimentary tone by producing sucking movements (Exp. No. 18 of Dec. 17). This absence of differentiation continued until the age

of the infant approached 2½ months. Then a slight difference between the reactions to the conditioned and the differentiated tones began to appear in some of our experiments.

However the weakening of the sucking movements (which had formerly been very expressive) offered considerably difficulties in further experiments. In Exp. 45 of Jan. 14 and 46 of Jan. 15, referring to the age of 2 months and 26 days, it was still possible to note the presence of the differentiation. The differentiation obtained became very unstable, and further weakening of the sucking movements did not allow us to ascertain whether it was firmly established.

The period of firm differentiation of No. 1 can be noted in the infants described as occurring at the following ages found in Table IV.

TABLE IV

AGES FOR DIFFERENTIATION NO. 1 (11½ TONES)

Names	Age at the Beginning of the Differentiation	Number of Stimuli	Age at the End of Period
1. T. R....	2 mo. 14 days	29	3 mo. 14 days
2. G. P....	1 mo. 2 days	55	2 mo. 10 days
3. L.E.....	1 mo. 11 days	34	2 mo. 25 days

Experiments proved the first of these infants at the age of 4 months and 4 days capable of discriminating sound differing by 5½ tones. More subtle differentiations of sounds (differing by 3½ and 2½ tones) of a similar degree of distinctness could not be obtained.

During the process of the development of a differentiation three main stages can be noted:

1. A period of absence of differentiation, during which the infants responded to the differentiated tone, not accompanied by feeding, in the same way as to the conditioned tone, by sucking movements.

2. A period of unstable differentiation, when (*a*) a lessening of the effect of the stimulus differentiated in comparison with the effect of the conditioned stimulus is noted; (*b*) separate combinations in the course of one experiment as well as on different days, show a complete absence of any alimentary

reaction to the stimulus differentiated, which again is followed by an alimentary reaction; (*c*) the differentiation appearing in this period is rather unstable, depending much upon the general excitability of the infant.

3. A period of stable differentiation. In this period the infants clearly enough display a discrimination of the two stimuli; a definitely formed differentiation, which if not maintained by repetition, disappears after a few days but can be easily restored. No matter how firmly it may have been established, the differentiation may show sudden vacillations.

Summary and Tentative Conclusions

1. The conditioned alimentary reflexes, in response to auditory stimuli, appear in the first half of the second month of the human infant's life.

2. There are three stages in the development of the conditioned auditory reflexes: (*a*) the stage of indifference, (*b*) the inhibition of movements and the appearance of the first signs of a conditioned reaction; (*c*) the stage of a clear conditioned reaction.

3. In the formation of the conditioned auditory reflex the main rôle is played by the child's age and not by the number of stimulations.

4. The sucking movements are the most convenient and simple indication of the conditioned alimentary reflexes in the first three months of the child's life.

5. At the age of 2½ to 3½ months the child is able to differentiate as small a difference as 11½ tones as indicated by the method of conditioned reflexes.

6. At the age of 4 months and 4 days the differentiation amounts to 5½ tones.

7. There are considerable individual variations even in the formation of the simplest auditory differentiation.

8. In its formation the first differentiation formed passes through the following stages: (*a*) an absence of differentiation; (*b*) the stage of unstable differentiation and (*c*) the presence of differentiation.

9. The formed auditory differentiation being stable enough

still may disappear under the influence of certain outer and inner factors.

10. The presence of a differentiation suggests a complication of the cerebral functions with the age of the child.

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MEDIATE ASSOCIATION [1]

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I. INTRODUCTION

That the conventional laws of association between ideas were in need of some form of supplementation was recognized early, and the concept of mediate association was suggested. In his *Treatise of Human Nature* Hume made the following observation: "That we may understand the full extent of these relations, we must consider, that two objects are connected together in the imagination, not only when the one is immediately resembling, contiguous to, or the cause of the other, but also when there is interposed between them a third object, which bears to them any of these relations." [2] Hartley also saw the need of some such concept to explain all the apparent relationships between ideas.

Hamilton, however, contributed the first clear analysis of the concept and also related the now classical example of mediate association from his own experience.[3] He maintained that the successive appearance of apparently unrelated ideas in consciousness can be accounted for only on the basis of a third idea which is definitely related to both of them. This third idea may be (1) conscious for an instant and immediately forgotten (as Stewart maintained) or (2) unconscious. Hamilton felt in agreement with Leibnitz concerning the existence of unconscious ideas, and accordingly took the latter alternative.

According to Hamilton's analysis, the term 'mediate association' refers to an indirect associative connection be-

[1] The writer is indebted to Dr. Harvey A. Carr for suggesting the problem and first method used in this dissertation and for valuable criticisms of the first manuscript.

[2] D. Hume, *A Treatise of Human Nature*, p. 320, London, Longmans, Green and Co., 1886.

[3] Sir W. Hamilton, *Lectures on Metaphysics*, Vol. I, pp. 351–354; Vol. II, pp. 244–245, Edinburgh and London, William Blackwood and Sons, 1859.

tween two items of experience, ideas, or mental acts, as opposed to a direct type of associative connection. And this indirect associative connection is one supposedly mediated by a third item that has been directly associated with both of them. When we find two ideas *A* and *B* appearing in succession with no possibility of their previous, direct association, we will usually find that both *A* and *B* have formerly been associated with an item *C*.[4] Thus, the connection *A*—*B* is the result of two other connections, *A*—*C* and *B*—*C*. However, in spite of this very definite statement of the meaning of the term, it is noteworthy that in the writings on so called 'mediate association' the term is used in various ways.

In the first place, there is the question of whether the common mediating item is conscious or not. On the one hand, some writers insist that the crucial point is that the common item be unconscious; and others, that a mediate association is one in which the mediate factor occurs in consciousness but is only dimly perceived or unnoticed. Hamilton held the former view. The latter view was accepted by Wundt.[5] Apparently, according to Wundt, in mediate association there is no *A*—*B*, but an *A*—*C*—*B* connection.

Some writers have used the term to express any associative connection that is mediated by a physiological, or neurological, and, therefore, unconscious element. When *A* and *B* appear together in consciousness, the neural elements with which they are correlated are joined by the neural element of *C*. This definition really amounts to a neurological statement of Hamilton's standpoint. Atherton and Washburn use the concept in this sense.[6]

Titchener, as Atherton and Washburn point out, uses the term in the physiological sense, but in a fashion that is unique.[7]

[4] Sir W. Hamilton, *Lectures on Metaphysics*, Vol. I, pp. 351–354, Edinburgh and London, William Blackwood and Sons, 1859.

[5] W. M. Wundt, Sind die Mittelglieder einer mittelbaren Association bewusst oder unbewusst? *Philosophische Studien*, 1894, **10**, 326–328.

[6] M. V. Atherton and M. F. Washburn, Mediate associations studied by the method of inhibiting associations: an instance of the effect of 'Aufgabe,' *American Journal of Psychology*, 1912, **23**, 101–109.

[7] E. B. Titchener, *A Text-Book of Psychology*, p. 386, New York, Macmillan Co., 1910 (revised).

The following hypothetical example, to be found in his textbook, explains what the concept means for him: An individual, on two separate occasions, observes two pictures which are done by the same painter; in neither instance does he consciously apperceive the painter's name in the corner; the second picture, nevertheless, reminds him of the first. The signature of the painter has acted as mediator between the two pictures. In neither instance was the signature conscious, but it must have left an impression on the nervous system which, when aroused, served as mediator between the two conscious items on the second occasion. No other writer or investigator in the literature has used the term in excactly this sense.

Still another sense in which mediate association has been very frequently used is that of the arousal of an idea by a present, but unnoticed, sensory experience. The chief difference between this definition and Titchener's is that it is not necessary that the sensory stimulus be one that has never been conscious. This definition accounts for the arousal of *B* in terms of an unconscious *C*, rather than the connection between *A* and *B* in terms of an unconscious *C*.

Mediate association has also been used in the sense of remote association, or in the sense of 'mediate successive association' that Ebbinghaus deals with in his *Ueber das Gedächtnis*.[8] Remote association refers to an associative connection between *A* and *B* by virtue of the fact that they have been learned in the sequence *A*—*C*—*B*. For mediate association, the previous associations need not necessarily be successive ones. Although certain writers have used the term mediate association in the sense of remote association, they have recognized that they were using it in a sense different in certain respects from previous usage.[9]

This divergence of opinion as to the nature of mediate association is attested by Piéron in his theoretical discussion of the subject in the *Revue Philosophique*, 1903.[10] The chief

[8] H. Ebbinghaus, *Ueber das Gedächtnis*, p. 139, Leipzig, Duncker & Humblot, 1885.

[9] See, *e.g.*, F. Nagel, Experimentelle Untersuchungen über Grundfragen der Assoziationslehre, *Archiv für die gesamte Psychologie*, 1912, **23**, 156–253.

[10] H. Piéron, L'association mediate, *Revue Philosophique*, 1903, **56**, 142–149.

divergence in defining the term has been over the question of whether the common item is consciously present during the appearance of the mediate association or not. Hume, Hamilton, Myers,[11] and the majority of the experimenters have leaned toward the opinion that defines mediate association in the narrower sense as an indirect association between items, the common term being non-conscious. Wundt, Aschaffenburg, and others have used the same terms to describe an indirect association between two items, the intermediate, common item being conscious. The latter view holds that the common item is conscious only to a slight degree, unattended to, or not apperceived.

The usages and definitions of the term mediate association thus differ. Since definitions of terms are arbitrary, it is futile to select any one of these definitions as the true and correct one. Consequently we shall select three typical definitions of the term, and discuss the truth of each of these three concepts in the light of our experimental findings.

1. The first may be termed an objective definition. Will two items, *A* and *B*, tend to recur together or in immediate succession in virtue of the fact that both have been previously associated with a common item *C*? It is to be noted that this definition makes no assumptions as to the mechanism of arousal; it makes no assumptions as to the presence or absence of *C* at the time of recall; and it makes no definite assumptions concerning a stimulus-response relation between *A* and *B*.

2. Because of their previous association with a common item *C*, will the presence of *A* arouse *B* when *C* is not present? This definition differs from the first in that *A* is assumed to be the stimulus which arouses *B*, and that *C* is not present at the time of recall. This definition of the term is the one adopted by Hume and Hamilton.

3. When two items *A* and *B* have been associated with a common item *C*, will thereafter the presence of *A* first arouse *C* and thus indirectly arouse *B*? This definition differs from the second in respect to the mechanism of arousal. *B* is

[11] C. S. Myers, *A Text-Book of Experimental Psychology*, pp. 159–160, Cambridge, University Press, 1922.

aroused by the presence of *C* and *C* in turn was aroused by *A*. The stimulus-response sequence can thus be represented by the symbol *A*—*C*—*B*.

The concept of mediate association does not assume that *B* will recur every time that *A* is present. In the course of experience the item *A* has become directly associated with a great many items, and naturally *A* is more likely to revive some of these items rather than any of those with which it is mediately connected. The concept presumes that mediate revival will be an occasional event which occurs only under favoring circumstances. What are these favoring circumstances? Will it occur only for certain materials—logical material as opposed to nonsense material? Will it occur more frequently for certain individuals—for example, those who have developed certain habits of thought? Will it occur only for certain behavior situations which involve a predisposing mental set at the time of recall? Any experimental test of the truth of the concept must take into consideration the nature of these predisposing conditions.

Experimental tests of mediate association, with one exception, have given negative results.[12] The general method used has been as follows: The subject was given one exposure of a list made up of pairs of *A*—*C*'s and *B*—*C*'s, the *C* items being common to two pairs. The subject was then presented with a list of *A*'s or *B*'s or both and instructed to name the first idea coming to mind. The resulting amount of mediate association was compared with the amount allowed by chance.

These experiments were defective in two respects. (1) The paired items were exposed but once, and there was no subsequent recall test to ascertain whether these associations were established or not. At best only a few associations could have been established and these not very strongly. There is thus no guarantee that these basic connections were effectively established. (2) They assumed that the two items that were presented with the common item had never been directly connected with each other in times past, but the only evidence for this assumption is that of memory. A control

[12] See references.

experiment should have been performed upon another group of subjects in which the final recall test was given without the previous exposures of the pairs. The number of 'correct' recalls found by such an experiment would be the result of chance plus the direct associations which have been formed between the items by previous experience.

II. THE EXPERIMENTS

Part I

This experimental section is divided into two parts. In the six experiments to be described in the first part, one general method was used. Two lists of paired associates were formed, one of the items of every pair in one list being identical with one of the items in the other list. One list, thus, was a series of *A*—*C*'s; the other, a list of *B*—*C*'s. Each pair in both of the lists was composed of a common item and a non-common item. In making up these lists, the same kind of material was not used for all items: the *A*'s, the *B*'s, and the *C*'s were each of a different type of material. For instance, in one experiment, words were used for *A*'s, letters of the alphabet for *C*'s, and numbers for *B*'s. Thus, the first list was a series of words paired with letters; and the second list was a series of numbers paired with the same letters. The subject was required to memorize both of these lists in succession by the paired associate method. According to this method, each exposure of the list of pairs was followed by an exposure of a list of the instigating items. When the single items alone appeared, the subject attempted to name as many of the associated items as possible. These alternate exposures of the paired and single items were continued until the subject had memorized to the point of three perfect recalls. After the second list was learned, a list of single, non-common items of one of the lists of pairs was exposed and the subject was given instructions to name the first item of the class of material that made up the non-common items of the other list that comes to mind. In short, after learning an *A*—*C* list and a *B*—*C* list, the subject is presented with the list of *A*'s and instructed to name a *B*. For instance, to return to the above example, after learning

paired words and letters, and paired letters and numbers, the subject is presented with the list of words only, and instructed to name the first number coming to mind.

In each of the following six experiments introspective reports were taken in the form of answers to definite questions. The questions were formed in such a fashion as to determine whether the subject was (1) aware of the mediate, common, item while making his recall and whether or not he was (2) aware of the nature of the set-up of the two lists he had memorized in succession, *i.e.*, that according to the arrangement two different items were being associated with an identical common item. It is a fair assumption that if the subject is not aware of the second factor he did not directly associate, during learning, the pairs of non-common items according to identical common item.

Experiment 1

Procedure.—The subject first learned a list of twenty paired words and letters. The words were in the position of stimulus; the letters, in the position of response. Immediately afterwards, he learned in the same manner a list of twenty pairs of numbers and letters. The letters were the same ones used in the first list; the numbers were from one to twenty. The letters were in the position of response. Thus, the common item for these two lists was the response. The pairs were exposed on an electrically operated memory drum. The exposure time for each pair was two seconds. Place associations were eliminated by presenting the materials in random orders. The criterion of learning was three perfect trials in succession. Twenty-four hours after the completion of the second list, the subject was presented with the list of words, and instructed to name the first number between one and twenty that came to his mind. The subject was asked not to use any plan of naming numbers, such as frequent repetition of the same number, or naming odd or even numbers.

When each subject had completed the experiment, *i.e.*, had finished the forced recall twenty-four hours later, he was asked the following questions. (1) Did the numbers come with difficulty? (2) Were you aware of the letters that you had associated with these words when the words appeared? (3) Were you aware, while memorizing the two lists of pairs, that both a word and a number were being associated with each etter?

The attempt to inhibit the direct association that has been formed during the learning of the two lists by positive instructions to recall an item of a different class is considered the important innovation in method here. Thus, the instructions will inhibit the direct connection, *A*—*C*, by calling for an item of class *B*, where there is an item that has been mediately connected with *A*. If the mediate connections exist, this method should force them to appear. However, this procedure differs in other, perhaps equally, important respects from those used in previous experiments on the problem. For one thing, the basic associations between the two items and the common

one are learned by the present procedure, and, therefore, known to exist; whereas in the previous experiments a single exposure of the paired items was relied upon for the formation of the basic associations. Having the paired items memorized (or presented) in two separate lists is another point of difference from the older procedure, which presented all the items in one list. More subjects were used in the present experiment than were used in the previous studies. Any one subject was used only once in the present study; whereas in previous ones the same subjects were used in several experiments. And the number of paired items to a list is much larger than was used in any of the other experiments. These last two factors, namely, increased number of subjects and increased number of pairs, both should operate to decrease the chances of direct association between the non-common items, during learning or memorizing.

Results.—Table 1 shows the results obtained from this experiment. In this table the number of the subject is on the

TABLE 1

Subject	No. of M.A.	Subject	No. of M.A.
1	0	11	1
2	12	12	1
3	1	13	2
4	2	14	1
5	4	15	0
6	0	16	0
7	0	17	1
8	0	18	1
9	2	19	1
10	8	20	0

left; the number of mediate associations, on the right. The mediate associations were those instances in which the stimulus word and the reaction number had both been associated with the same letter. Out of 400 associations, or forced recalls, only 37 were mediate, or 9.3 percent, and 20 of these mediate associations, or over half, were given by two subjects: number 2 and number 10.

The introspective results in the form of answers to the three questions were as follows. The subjects numbered 3, 9, 12, 13, 14, 16, and 19 answered the first question in the affirmative, *i.e.*, that the forced recall of numbers came with difficulty. Subjects 3, 5, 7, 8, 9, 10, 11, 12, 13, 15, 16, and 19 answered 'yes' to the second question, affirming that they were aware of the common letters when the words were presented. And in answer to the third question the following subjects reported that they were aware that both a word and a number were being memorized with the same letters during learning of the two lists: 2, 4, 9, 10, 11, 13, 16, 17, 20.

The significance of these introspective results will be discussed under the summary of results for the first four experiments.

Experiment 2

Procedure.—The procedure and material were the same as those used in Experiment 1, except that the lists were shortened to 16 pairs of items, and the forced recall was taken immediately after the learning of the second list. Ten subjects were used. These subjects were asked the same questions at the termination of the experiment.

Results.—Table 2 shows the number of mediate associations given by each subject. Out of a possible 160 mediate associations, only 5 were mediate, or 3.1 percent.

TABLE 2

Subject	No. of M.A.	Subject	No. of M.A.
1	0	6	0
2	1	7	0
3	0	8	0
4	0	9	1
5	1	10	2

In answer to the three questions, subjects 2, 3, and 4 reported that they had difficulty naming numbers during the forced recall. Subjects 2, 3, and 4 reported that they were aware of the letters during the recall test. (Subjects 5, 8, and 9 reported that they were aware of the letters in a few cases.) And subject 1 was the only one to report that he was aware of the common item factor during the memorizing of the two lists.

Experiment 3

Procedure.—Here the procedure was the same as in the two preceding experiments, except that the material was exposed for a longer period, 4 seconds, on large cards that were turned by the experimenter. One list, again, was made up of paired words and letters; the other, of paired numbers and letters. The common item was the response. There were 16 pairs to the list. Ten subjects were used. The forced recall, in which the subject was asked to recall numbers to words, was taken twenty-four hours later, as in Experiment 1.

After completion of the forced recall, the subject was asked the following two questions: (1) Did any of these numbers seem to 'belong' with the words with which they recurred? In other words, were any of the words and numbers closely associated? (2) Did you refer at any time during the recall to the associations that you formed yesterday?

Results.—The number of mediate associations made by each subject is indicated in Table 3. As can be seen in this

TABLE 3

Subject	No. of M.A.	Subject	No. of M.A.
1	1	6	0
2	4	7	3
3	1	8	1
4	4	9	1
5	0	10	2

table, out of a possible 160 associations (or forced recalls), 17 were cases of mediate association, or 10.6 percent.

In answer to the questions, subject 2 reported 'belongingness' in three cases; subject 4, in four cases; subject 7, in one case; subject 9, in two cases; and subject 10, in one case. All of these cases were correct, *i.e.*, mediate association, except one of those indicated by subject 9. In answer to the second question, only two subjects reported a reference to the associations made previously: (1) Subject 4 reported noticing in four cases, during learning, that a particular word and number were being associated with the same letter, and 'carried them over' to the following day; (2) subject 10 reported the same for the one association of which he was certain of 'belongingness.'

Experiment 4

Procedure.—The procedure was the same as that used in Experiment 3. In this experiment, however, the letters (common items) were in the position of response in the first list when paired with words, and in the position of stimulus when paired with numbers in the second list. Thus the first list was an *A—C* list, and the second one was a *C—B* list. Ten subjects were used; and recall of numbers to words was taken twenty-four hours later.

After completion of the forced recall, these subjects were asked the same two questions that the subjects of Experiment 3 were required to answer.

Results.—Table 4 shows the number of mediate associations given by each subject. Thirty-nine associations out of 160 were cases of mediate association, or 24.4 percent. However, all but three of these cases were made by three subjects: 2, 3, and 4.

TABLE 4

Subject	No. of M.A.	Subject	No. of M.A.
1	2	6	0
2	16	7	0
3	8	8	0
4	12	9	0
5	0	10	1

Subject 2, who recalled the correct number in every case, reported that he was aware while recalling that in each case the number and word had been associated with a common letter on the previous day. Of the eight mediate associations given by subject 3, seven were cases in which he knew that the number and word had both been associated with the same letter on the preceding day. And subject 4 reported that he was certain of the connection with a common letter in twelve of the sixteen cases; these twelve were the ones that his record shows as correct, mediate associations.

In answer to the first question, subject 1 reported 'belongingness' in four cases (one of which was mediate association); subject 5 reported one case; subject 6, three; subject 7, two. In answer to the second question, subject 1 reported that he was aware of the connection with a common letter on the preceding day in four cases (only one of which was correct); the remaining subjects (excluding, of course, 2, 3, and 4) denied any reference to associations formed on the preceding day. Many of the cases of belongingness were explained by the subject on a basis extraneous to the experiment.

Chance.—When the list of words was exposed to a subject in the above four experiments, he was instructed to name a number between one and twenty, inclusive. Now, on a basis of pure chance, the probability is one out of twenty that any one association will be correct (since only one of the twenty numbers has been connected with the same letter as the word). And regardless of how many associations, or guesses, were made, one twentieth, or 5 percent, of them should be correct associations. However, there are a number of differences between a pure chance situation and these experimental situations which would tend to increase this percentage, making the chances of correctness larger than 5 percent. For one thing, there is the possible prior connection between some of the words and some of the letters which would not exist in a pure chance situation. However, the chief one of these differences is that in the experimental situation repetition of the same number was inhibited (though not prohibited), while in the pure chance situation this favorable factor does not operate.

In order to determine more exactly what the psychological chance would be, the following control experiment was performed. The list of twenty words used in the experiments was read to a group of 144 individuals, who were under instructions to write down immediately upon hearing a word the first number between 1 and 20, inclusive, that came to mind (these instructions were made as nearly as possible like those that were used with individual subjects in the experiments). 2,880 controlled or forced associations were thus obtained, and 195 of them were found to be 'correct.' The percentage of correctness is 6.8. The average number of correct responses or associations per individual was 1.4, and the range was between 0 and 5. Only one person gave five correctly, however, and only 28 of them gave no correct numbers.

Summary of the Results Obtained in the First Four Experiments.—In Table 5 the results of the above four experiments

TABLE 5

Experiment	No. of Subjects	No. of Associations	No. of Mediate A	% of Mediate A
1	20	400	37	9.3
2	10	160	5	3.1
3	10	160	17	10.6
4	10	160	39	24.4

are summarized, including all of the subjects. The first column of this table contains the number of the experiment; the second column contains the number of subjects; the third column, the number of recall associations; the fourth column, the number of mediate associations; and the fifth column, the percentage of mediate associations. The total number of subjects is 50; the total number of recall associations is 880; the total number of mediate associations is 98; and the percentage of mediate association for all four experiments combined is 11.1.

If 6.8 be taken as the percentage of mediate association that chance would allow, then the objective results of three of these experiments show mediate association: Experiments

1 and 3 to only a slight degree; Experiment 4, a fairly large amount. Experiment 2, on the other hand, shows a percentage of mediate association much smaller than the percentage of correct associations in the control experiment. The records of the subjects in Experiments 2 and 3 are relatively consistent; 1 and 4, however, are characterized by a few atypically large records. Another noteworthy feature of these data is that the proportion of subjects giving zero mediate associations is much larger than to be expected on a basis of chance and much larger then the proportion of subjects giving no correct associations in the control experiment.

The important subjective results are based on the answers made to questions (2) and (3) in Experiments 1 and 2 and on the answers made to question (2) in Experiments 3 and 4. These questions were aimed at determining (1) whether *C* was ideationally present during the recall of *A*—*B*, and (2) whether *A* was ideationally present during the learning of *B*—*C*. In Experiments 1 and 2, both of these possibilities were sought for by questions (2) and (3). In Experiments 3 and 4, however, the latter possibility only was determined by question (2).

In Experiment 1, twelve subjects answered in the affirmative to question (2), *i.e.*, that *C* was ideationally present during the recall test; these subjects gave 20 of the total 37 mediate associations. Nine subjects answered in the affirmative to question (3), indicating the ideational presence of *A* during the learning of *B*—*C*; these subjects gave 28 of the total 37 mediate associations. Of the two atypical subjects, 2 and 10, subject 10 answered both questions in the affirmative, while subject 2 answered only to question (3) with affirmation. These data indicate that the second possibility of association between *A* and *B* with *C* present, *i.e.*, ideational presence of *A* during the *learning* of *B*—*C*, is the efficacious one. This conclusion is confirmed when we turn to the subjective data of Experiment 2, which did not show objective mediate association. Here, six subjects reported the ideational presence of *C* during recall of *A*—*B*, or affirmed question (2), while only one of the total ten subjects reported the presence of *A* during the learning of *B*—*C*, or affirmed question (3).

Of the ten subjects used in Experiment 3, two answered in the affirmative to question (2), *i.e.*, indicating the presence of *A* and *B* with *C* during the memorizing of the second list of pairs. These two subjects gave six of the total 17 mediate associations. In Experiment 4, four subjects answered in the affirmative; these four subjects gave 38 of the total 39 mediate associations obtained in the experiment. These data confirm the conclusion based on the subjective data of Experiments 1 and 2, namely, that the disparate items *A* and *B* are associated *through C* during the *learning* of the second list of pairs.

The two outstanding features of the objective results of these experiments are the large proportion of subjects giving no mediate association and, in contrast, the large amounts of mediate association as shown in the records of a limited few subjects, particularly subjects 2 and 10 in Experiment 1 and subjects 2, 3, and 4 in Experiment 4. And turning to the subjective data, we find that while all of these five subjects giving large amounts of mediate association are in the class of subjects that we assumed probably associated *A* and *B* during learning, three of the subjects giving no mediate association are also in that class. The probable explanation of this apparent contradiction is that the questions were aimed at determining a condition without which the association of *A* and *B* could not have occurred, namely, awareness of the common series of items in the two lists. However, it does not necessarily follow that all subjects who were aware of the relationship should have formed the connections of *B* with *A* through *C*. Some subjects reported that they were not aware of the fact that the same letters were being used in the two lists; some reported that they were aware of the common letters but made no attempt to connect the words and numbers. We may assume that the five subjects who gave large amounts of mediate association did make this attempt, while the three subjects giving no mediate association but reporting awareness did not make the attempt.

That such an attempt at associating *A* and *B* through *C* during learning would result in a large amount of mediate association was confirmed in the case of two subjects whose

records are not included in the table. These individuals were told specifically the nature of the set-up and instructed to attempt to connect the words and numbers accordingly. These subjects were put through the experiment in the same manner as the others. One of these subjects' records gave 7 instances of mediate connection out of a possible 20. The other's record was one of 16 mediate connections out of a possible 16.

The data of the four experiments justify the following conclusions:

1. For our experimental conditions, there is a tendency for two items to recur together when both have been previously associated with a common item. The occurrence of this objective type of mediate association is limited to a few individuals and to certain of the experimental conditions.

2. Such mediate recall is usually a function of the presence of the common item, and it generally occurs during the memorization of the second list. It is to be noted that the revival of the absent item by the presence of the common item is a case of backward association.

3. It is highly improbable that mediate recall ever occurs without the presence of the common item.

Experiment 5

Procedure.—In the experiments described thus far, mediate association has been tested with verbal material. In this experiment an attempt was made to test the concept with motor, or skill material. A card-sorting board with twenty-five compartments on each surface was used. The compartments were identically arranged on both sides. Two decks of twenty-five cards were made: one deck of numbers one to twenty-five, and another deck of twenty-five letters. Each of the slots on one side of the board was marked for a number; and each slot on the reverse side was similarly marked for one of the letters. The numbers and letters on the compartments were arranged in a random order. After brief instructions as to the nature of the performance, the subject sorted each of the two decks of cards over the board thirty times. The subject first sorted one deck thirty times, rested five minutes, and sorted the second deck thirty times. The deck of cards was shuffled by the experimenter before each trial. Half of the subjects sorted the letters first, half the numbers. Twenty-four hours later the subject was presented with the deck of numbers and told to pair each number with one of the lettered cards, which were arranged alphabetically on a table, on the basis of the common slot into which they were sorted the day before. During this procedure the board was not in sight. Ten subjects took part in this experiment.

Under these conditions, the numbers and letters were connected with the same

movements of sorting, both a letter and a number being connected with each particular movement. Another common factor was the same location in the visual pattern of the board. When the subject had finished pairing the numbers and letters he was asked to indicate the pairs the correctness of which he was certain and also to describe the method that he had used in pairing.

Results.—The significant data of this experiment consist of two figures for each subject: (1) the total number of correct pairings of numbers and letters, and (2) the number of correct pairings of which he was not certain. These data are shown in Table 6. Column I includes the number of the subjects;

TABLE 6

I	II	III
1	12	2
2	18	3
3	5	2
4	2	0
5	6	1
6	3	2
7	9	0
8	1	1
9	16	7
10	4	2

column II, the number of correct pairings; and column III, the number of correct pairings of which the subject was not certain. The ten subjects made in all 250 pairings of letters and numbers (25 per subject). Of these, 76 were correct on the basis of common slot, or 31.0 percent; and of these 76, 20, or 10.3 percent were cases in which the subject was not certain. On the basis of pure chance, 4.0 percent of the pairings would be correct.

The subjective reports indicated that the certainty with respect to particular pairs of letters and numbers was based on the definite knowledge that they had been placed in the same slots on the preceding day. (Thirty of these instances of certainty were correct.) The subjects' reports of the methods used in making the pairings indicate that they retained an image of the board and had made their pairings on the basis of inference from this. Thus, the pairs that had been sorted in prominent slots, such as the corner ones or the center slots in the side rows and columns, were rarely missed. The remaining items were paired by a process of elimination, in which the subject paired first the ones that he was most

certain of, and so on until the last few were pure guesses. None of the numbers and letters seemed to the subjects to 'belong' together.

The chief value of this experiment lies in the fact that it shows that rather large amounts of mediate association in the purely objective sense may occur under favorable circumstances. With reference to the mechanism involved, however, very little can be concluded. That most of the mediate associations in the objective data belong to the third class as previously defined is apparent. The subjects' reports indicate that the common items (slots) were present during the mediate associations (correct pairings). The number of correct pairings in which the subjects were uncertain, and, hence, in which the presence of *C* may be doubted, are due either to mediate association in the second sense or to chance. The proportion of cases is larger than that allowed by a pure chance situation. But there are a number of differences between this situation and pure chance, besides the element of certainty in some cases, which would increase the chances in the experimental situation. Chief among these differences is the method of pairing by elimination which was used by the subjects, and which would not operate in a pure chance situation.

Thus, we may conclude that this experiment offers no evidence from which mediate association in the sense given in definition (2), that is, without the presence of the common item, may be inferred.

Experiment 6

Procedure.—Lists of pairs of nouns were the materials used. Each list contained sixteen pairs, which were exposed in random order on a memory drum. The exposure time per pair was three seconds. The subjects were divided into two groups. The first group (the test group) learned two lists of pairs by the paired associate method described in Experiment 1. One list was a series of *A*—*C*'s; the other, a series of *B*—*C*'s. The *B*'s were related to the *A*'s as part to whole, each whole and its part being paired with the same common *C*. Thus, one list was a series of wholes paired with words; the other, a series of parts paired with the same words, a whole and its part in the other list being paired with the same common word. For example, one pair in the first list was 'house—dog'; and the corresponding pair in the second list was 'room—dog.' The subject learned these lists in succession, and immediately after completion of the second was presented with the list of single words representing wholes and instructed to name the first part that came to his mind. He was not told that parts and wholes made up the lists he had learned, but was merely told to recall with a whole-part set.

The second group (the control group) learned two similar lists of paired nouns. With the exception of sixteen different words which were substituted for the *C* items in the second list, the words were the same ones used in the test group. Thus, these subjects learned a list of wholes paired with words; and a second list of parts paired with different words, *i.e.*, a list of *A—C*'s and a list of *B—D*'s. The procedure followed with the control group was exactly the same as that of the test group. Twenty subjects were used in each group.

The following whole-parts were used in this experiment: shool—class, tree—leaves, car—fender, road—curb, table—leg, camera—lens, hat—band, office—desk, house—room, lamp—shade, dresser—drawer, shoe—string, gun—trigger, horse—neck, piano—key, skate—wheel. Most of these are more or less closely associated in everyone's mind; and if an individual were given the list of wholes and instructed to name parts, he would undoubtedly name several of the parts that are paired in this list. The assumption upon which this procedure is based is that if any difference is found in the number of correct associations given by the two groups it can only be caused by the one difference between the two groups, namely, that in the test group the whole and part are associated with a common item while the same whole and part are associated with different items in the control group. In other words, the difference would be the effect of mediate association.

In this experiment introspective reports, in the form of answers to definite questions, were taken for the test group only. If *C* is reported as present, this fact indicates the truth of the third definition. If *C* is reported as absent, then, presumably, the second definition is true.

Results.—The results of this experiment are presented in Table 7. The first column of this table gives the number of subjects in the test group; and column two, the number of

TABLE 7

Test Group		Control Group	
Subject	No. of M.A.	Subject	No. of M.A.
1	7	1	8
2	10	2	8
3	15	3	5
4	15	4	9
5	11	5	8
6	8	6	6
7	5	7	7
8	13	8	6
9	11	9	5
10	14	10	5
11	4	11	6
12	10	12	8
13	9	13	8
14	10	14	6
15	9	15	6
16	14	16	4
17	14	17	5
18	13	18	4
19	13	19	5
20	9	20	8
Total	214		127
Average	10.70		6.40
Sigma	3.12		1.49

correct associations given by each subject. The third column gives the number of subjects in the control group; and the fourth column, the number of correct associations given by each subject. The average number of correct associations for the test group is 10.7, while the control group has an average of 6.4 correct associations. The difference between these means is 4.3; the sigma of the difference is .774. Hence, the difference between the means is 5.5 times as large as the sigma of the difference. This reliable difference is objective evidence of mediate association in the sense of the first definition.

The answers that the subjects of the test group made to the following questions comprise the subjective data obtained in this experiment. (1) Did the parts come with ease? Only five of the twenty subjects failed to answer in the affirmative. These subjects are numbered in the table: 4, 5, 7, 11, and 15. (2) Were you aware during the memorizing of the two lists of pairs that one series of words was common to both, and that you were associating two different words with each of these common words? Every subject in the group answered this question in the affirmative. (3) During the learning of the second list of pairs, were you aware of the whole-part relationship existing between one series of words in each list? Every subject but one answered this question with the affirmative. This exceptional subject is numbered 8 in the table. (4) During the recall test, that is, during the naming of parts to wholes, were you aware of the words which you had previously associated with the wholes? Only six subjects answered this in the negative; they are numbered in the table: 1, 6, 8, 10, 11, 19.

These subjective data indicate the strong probability that the wholes and parts were associated through the common words during learning, *i.e.*, that the mediate associations occurred during learning with *C* present. Undoubtedly the whole-part relationship between the items facilitated their association during the learning of the second list of pairs. This factor in conjunction with two others accounts for the larger amount of mediate association being evinced in this experiment than was evinced in the preceding experiments.

These additional two factors are the meaningful nature of the material used in this experiment and the already existing connections between the items.

From the data of this experiment it may be concluded that mediate association in the objective sense occurs in large amounts under favorable circumstances. These data also indicate the occurrence of mediate association in the sense of the third definition: namely, that when two items are associated with a common item an association is mediated between them by the presence of the common item.

Part II

The remaining experiments that were performed on this problem are included in a part separate from the above experiments, because an altogether different method was used. Whereas in the previous experiments it was assumed that if a weak association existed between two items, forced recall would be influenced by that fact, in these experiments it is assumed that if a weak association exists between two items, they will be easier to learn together. Thus, these experiments are based on the following assumption: that if an associative connection is formed between two disparate items by virtue of their direct association with a common item, these two disparate items should then be easier to memorize than otherwise. This is essentially the same logic that is behind Ebbinghaus' test for remote association.

According to this method, the subjects are divided into groups and each group learns three lists of paired associates. The first two lists are the same for both groups: every pair in both lists consists of a non-common item and an item common to a pair in both lists, *i.e.*, the first two lists correspond to those used in the experiments described under part one. The third list is different for the two groups: (1) for the test group it is a list formed by pairing the non-common items from the first two lists on the basis of the common item; (2) for the control group, the third list is made up of the non-common items from the first two lists paired at random. Thus, the items used in the third problem are the same for both groups, the difference being that these items are paired differently.

If the test group learns the third list with greater facility than the control group, this can only be due to the already existing mediate connections between the items in the test group's third list.

Experiment 7

Procedure.—Simple, geometrical figures were used as materials in this experiment. The figures were drawn on cards which were exposed on a Guhin automatic multiple-exposure apparatus. Each list was made up of a series of paired figures. A pair consisted of a large and a small figure, the larger circumscribing the smaller. The exposure time was approximately four seconds per card (and per pair). The subjects were instructed to associate the larger with the smaller figure. After each exposure of the complete series of cards, the subject was presented with a sheet of paper on which the large figures only were mimeographed. The subject drew inside of these as many of the small figures as he could remember. The exposures of the series of cards were continued until the subject could draw all of them correctly and inside of the appropriate large figure. The same procedure was followed with all three lists, all of which were learned successively.

The first two lists, which were the same for both groups, contained ten pairs of figures. The smaller, inside, figures were common to both lists; the larger figures were different. Hence, the common items in these two lists were the responses. The third series was also made up of ten large and small figures, the large ones being the large figures used in the second series, the small ones were the large figures of series one. These figures were paired differently for the two groups. For the test group, they were paired according to the common small figure in the first two lists. For the control group, they were paired at random. Twenty-six subjects were used in each group.

At the completion of the experiment each subject was questioned far enough to determine whether he was aware during learning that common figures were being used or whether his idea of the nature of the set-up of the experiment was very definite.

Results.—In Table 8 the results in terms of trials (T) and errors (E) to memorize the three lists to a criterion of one

TABLE 8

Problem	I		II		III	
	T	E	T	E	T	E
Total—T. G.	167	781	126	503	161	678
—C. G.	166	724	117	467	120	415
Ave.—T. G.	6.42	30.04	4.85	19.35	6.19	26.08
—C. G.	6.38	27.85	4.50	17.96	4.62	15.96
Sigma—T. G.	2.59	17.09	1.09	5.42	2.10	11.75
—C. G.	2.87	19.75	1.72	9.88	1.27	7.51

perfect recall are given. In this table 'T. G.' stands for test group, and 'C. G.' for control group. The second row includes the total trials and errors for all three lists; the third

row includes the average trials and errors; and the fourth row, the sigma of the distribution. The differences between the two groups (test and control) for the first two lists are small and unreliable. For the third, however, the differences between the two groups are large, and are also reliable. In terms of trials: the difference is 1.57; the sigma of the difference is .48; and the difference is 3.30 times as large as the sigma of the difference. In terms of errors: the difference is 10.12; the sigma of the difference is 2.73; and the difference is 3.70 times as large as the sigma of the difference. These differences, contrary to expectation, are in favor of the control group, instead of the test group. Therefore, on the basis of these results, it is more difficult to connect two items that have been associated with a common item than it is to connect two items that have been associated with different items.

Some of the subjects used in this experiment were aware that a set of figures was common to the two lists, but they could not specify which figures these were. Only one subject was aware of the nature of the arrangement of the figures in the three problems, and he was in the control group. His data were not discarded, because they showed no effects of the knowledge. Most of the subjects were surprised when informed that the small figures used in the first two lists were the same. Thus, there is little room for doubting that these subjects (with one exception) did not ideationally connect the larger figures in the first two problems through the common smaller ones.

The reports of these subjects expressing complete ignorance as to the arrangement of the pairs, contrasts sharply with the reports of the subjects in the preceding experiments. The nature of the materials undoubtedly accounts for the difference. Most of the figures used in the lists were nonsensical in shape and, therefore, defied ideation. Hence, during the learning of the second list it was impossible for the larger figures (*A*'s) of the first list to be ideationally present and then associated with the large figures of the second list (*B*'s).

The conclusions of this experiment are negative for all of the three possible definitions of mediate association stated in the introduction.

Experiment 8

Procedure.—Peterson Rational Learning problems were used as materials in this experiment. These problems are composed of lists of paired items and, therefore, lend themselves easily to experimentation on association. The problems used in this experiment were twelve pairs in length. As in Experiment 7, the subjects were divided into two groups, and each group learned three lists. For the first list, the stimulus items were the months of the year, and the response items were the numbers 1 through 12. For the second, the stimulus items were the months of the year, and the response items were the letters A through L. For the third, the stimulus items were the numbers 1 through 12, and the response items were the letters A through L. The letters and numbers in the third list were paired differently for the two groups: with the test group they were paired according to the common month in the first two lists; with the control group they were paired at random.

The criterion of learning for all three lists was two perfect trials in succession. During the learning of each problem, the stimulus items were read to the subject in random order to avoid place associations. Twenty-three subjects were used in the test group; and twenty-four in the control.

This experiment differs from Experiment 7 in two respects: a different type of learning material is used, and the common items are in the position of stimulus in both lists, instead of response, as they were in Experiment 7. After completion of the third list, each subject was questioned in order to determine whether he had thought of the possibility of the letters and numbers in the first two lists being paired according to the common month in the third list. And also whether he had made any attempt to connect the letters and numbers accordingly during the learning of the second problem. Only four subjects answered these questions in the affirmative, and their records were discarded.

Results.—The first row in Table 9 shows the total trials and errors for the test and control groups on the three problems. The second row contains the averages; the third row, the standard deviations; the fourth row, the differences between the averages of the two groups; the fifth row, the standard error of the difference; and the last row, the difference divided

TABLE 9

Problem	I		II		III	
	T	E	T	E	T	E
Total—T. G.	152	558	125	408	130	487
—C. G.	137	503	126	419	134	485
Ave.—T. G.	6.61	24.26	5.43	17.74	5.65	21.17
—C. G.	5.71	20.96	5.25	17.46	5.58	20.21
Sigma—T. G.	2.32	13.50	1.41	6.30	1.40	7.64
—C. G.	1.43	8.09	1.62	7.41	1.47	9.85
Difference	.90	3.30	.18	.28	.07	.96
Sigma D	.56	3.26	.44	2.00	.42	2.56
D/sigma D	1.61	1.01	.41	.14	.17	.38

by the standard error of the difference. These differences are all small, and none of them is very reliable.

Therefore, the conclusions of this experiment are negative for all three possible definitions of the concept of mediate association.

The reports of the four subjects whose data were discarded furnish an interesting qualitative result to this experiment. When they were informed that the third problem was composed of a list of numbers and letters, they suspected that the pairing would be on the basis of a common month in the first two problems. Two of these subjects were on the control group and two on the test group. The two on the control group found that their suspicion was not true and finished the third list with an average record. The two on the test group, however, found that their suspicion was true and proceeded to learn the list by intentionally using the common item. Their records, in terms of trials, for the third problem were below the average for that list, and much longer than their records for the first two problems. In other words, it is more difficult to try to learn the list consciously by common item then merely to learn the list. One of these subjects made the statement that for him the numbers and letters were not associated at all; that he had to go each time from number to month and thence to letter. This fact is entirely congruous with the tendency implied in the results of Experiment 7: namely, that items that have previously been associated with the same item resist association themselves.

Experiment 9

Procedure.—The same procedure and materials that were used in Experiment 8 were also used in this experiment. The only difference between the two was that the lists learned in this experiment were arranged so that the common items, the months of the year, were the responses in the first problem and the stimuli in the second problem, while in Experiment 8 the common items were the stimuli for both problems. Twenty-five subjects were used in each group.

When the experiment was finished, these subjects were questioned in the same manner as those in Experiment 8. The records of three were discarded.

Results.—Table 10 is constructed on the same plan as Table 9. The differences between the test and control groups

TABLE 10

Problem	I		II		III	
	T	E	T	E	T	E
Total—T. G.	152	632	147	586	136	522
—C. G.	162	753	150	659	144	613
Ave.—T. G.	6.08	25.28	5.88	23.44	5.44	20.88
—C. G.	6.48	30.12	6.00	26.36	5.76	24.52
Sigma—T. G.	.84	6.80	.95	7.48	1.03	7.05
—C. G.	.90	10.14	1.13	8.95	.95	7.92
Difference	.40	4.84	.12	2.92	.32	3.64
Sigma D	.25	2.44	.30	2.34	.28	2.12
D/sigma D	1.60	1.98	.40	1.24	1.14	1.71

are all small and unreliable. Hence, the objective data of this experiment show no tendency toward mediate association.

This experiment confirms the results of Experiment 7 and 8. When the connection between *A* and *B* is tested by the method of relearning, there is no mediate association of any type.

III. SUMMARY AND CONCLUSIONS

The concept of mediate association assumes that two items that have been directly associated with a common item are, therefore, indirectly associated, and that this indirect or mediate connection will manifest itself in a tendency for the two items to recur together in *recall*. This has been tested in six experiments. In each of these experiments the previous functioning of the two items with a common one was insured by requiring the subjects to learn lists of common and non-common items to a definite criterion.

In these experiments, the test for the connection between the disparate items, or the effects of mediate association, was one of *forced* recall. The objective results of these experiments are: a positive effect in Experiments 1, 3, 4, 5, and 6; and no effect in Experiment 2. The subjective results consisted of reports on the part of the subjects indicating (1) whether or not the common item was ideationally present during recall, and (2) whether or not the disparate (non-common) items were associated during the learning of the second list. In both Experiments 1 and 2, a large proportion of the subjects reported that the common item was present during recall; a

large proportion of subjects in Experiment 1 and only one subject in Experiment 2 reported that the disparate items were associated during learning. The second factor is, therefore, the important one, since Experiment 1 offered positive effects and Experiment 2 offered no effects. The conviction that the disparate items were associated during learning of the lists, particularly the second list, is confirmed by the reports of the subjects of Experiments 3, 4, 5, and 6.

In the three remaining experiments, *relearning* was used as a test for the effects of mediate association. The objective results of these experiments are: no effect in Experiments 8 and 9, and a negative effect in Experiment 7. The subjective data of these experiments indicate that the non-common items were not associated during the learning of the second list.

All conditions were not held constant in the whole nine experiments. The following five conditions varied deserve special mention:

1. The position of the common items in the two lists that were learned, thus establishing the basic, primary associations. The common items occupied the position of response in both lists in Experiments 1, 2, 3, 5, 6, and 7. One of these experiments (number 6) gave a very large amount of mediate association; three of them (1, 3, 5) showed positive effects; number 2, no effect; and Experiment 7 showed a negative effect. The stimulus was the common item in Experiment 8, which showed no objective effects. The common item was the response in one list and the stimulus in the other in Experiments 4 and 9. Experiment 4 showed positive effects; Experiment 9 showed no effect. Apparently, there is no consistent correlation between position of common item and mediate associtaion.

2. The time interval between the learning of the two lists and the test for mediate association. This test was taken immediately after learning in Experiments 2, 6, 7, 8, and 9. A twenty-four hour interval was interpolated between learning and test in Experiments 1, 3, 4, and 5. Apparently, the effects of mediate association vary independently of this time interval.

3. The nature of the learning material. In the first place, memory (or verbal) material was used in every experiment but number 5. In this experiment a manual skill was used. The results of this experiment indicate to a high degree the effects of mediate association. The subjective reports indicate that the common items were verbalized, or carried in terms of ideas; thus, no conclusion can be drawn as to the influence of acts of skill as opposed to memory.

In the second place, the memory materials used in these experiments vary in degree of meaningfulness. The most meaningful materials were used in Experiment 6 (words in whole-part relationship); and this experiment yields the greatest amount of mediate association. Meaningful materials (words, letters, numbers) were used in Experiments 1, 2, 3, 4, 8, and 9; three of these show a positive effect and three no effect at all. Non-meaningful material (geometrical figures) was used in Experiment 7; and this experiment shows a negative effect. Thus, there is a tendency for the objective effects of mediate association to vary directly with degree of meaningfulness. Since meaningful material lends itself to ideation more easily than non-meaningful, this correlation could be inferred from the subjects' introspective reports.

4. The nature of the test used. The recall method was used as a test in the first six experiments; the relearning method, in the last three. Five of the first six experiments gave positive effects; only one experiment showed no effect. Two of the remaining experiments gave no effect; one showed negative effects. Thus, when the recall method is used, mediate association occurs; when the relearning method is used, it does not occur. According to the first method, the subject was presented with an item and instructed to name another item; in the second, he was presented with two items and told to connect, or memorize, them. In the former method, the subject is likely to cast about for some method of choosing an item to name. This would be association, but it might just as well be *through* the common item. In the second case, this possibility of choice is eliminated.

5. Individual differences in the matter of mediate associa-

tion are apparent. While the records of most subjects in the first six experiments show either no cases or very few cases of mediate association, a limited number of subjects gave a surprisingly large number of cases. These exceptions are, undoubtedly, due to individual differences in ideation, especially during learning. Some individuals, when learning, attempt to make only direct associations between successive items; others are thinking during learning, trying to detect indirect relationships. And this variation in type probably reduces to a fundamental difference in individuals very similar to the difference between the 'tough' and 'tender' minded individuals described by James.

The factors that favor mediate association are those that favor ideation. When two items have previously been associated with a common item, they later are associated themselves, because the presence of the common item, either sensory or ideational, has brought them together. Thus, the association is mediated *through* the presence of the common item.

Conclusions.—Any conclusions concerning the existence of mediate association will naturally vary with the definition adopted.

1. If the concept be defined as the tendency for the two items to recur together in recall, because they have previously been associated with a common item, the existence of this kind of mediate association is proven by our data.

2. If the term be defined as a tendency for one item to arouse the other in recall when the common item is not present at the time, the concept is false.

3. The third possible definition assumes that one of the two items will arouse the other when the common item is perceptionally or ideationally present at the time of recall. Our data support the truth of this concept.

The results of one of our experiments indicate that, when the relearning method and non-meaningful material are used, there is a tendency opposite to that of mediate association, *i.e.*, that when two items have been directly associated with a common item, they resist direct association themselves.

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COLOR-CONTRAST AND COLOR-CONSTANCY

BY GEORGE KATONA

This preliminary publication contains a short description of some of the experiments which I carried out during the last two years in Berlin and New York [1] with the purpose of understanding the relation between the phenomena of color contrast and the phenomena of color constancy. In various old as well as recent investigations it remained doubtful whether certain effects are to be explained by contrast or by constancy. By establishing criteria of distinction between contrast and constancy, we may expect to throw further light on the relationship between illumination and perceived surface color.

§ 1. "*Constancy Increases the Contrast*"

Figure 1 shows the plan of the first experiment. On a table stands a medium grey screen bent to an angle of 90°. One side of the screen, *A* 1, (30 × 25 centimeter) lies in shadow while the other side, *A* 2, is well illuminated. Behind the screen *A* there stands another screen *B*, of the identical color and in 'normal illumination.' In the centre of *A* 1 there is a small opening (1 cm square) through which part of screen *B* can be seen by the observer who sits at a distance of 3½ meters from the screen *A*. The observer can see no part of the screen *B* except that which is seen through the hole.

If one measures the quantity of light reflected by the different surfaces one finds the following relationship: *A* 2, the well illuminated sheet, is the brightest, then follows *B*, while *A* 1, the shadowed sheet, is the darkest. The judgments of the observers concerning the perceived brightness, however, are different: The brightest surface is *A* 2 and approximately equally bright is that small portion of *B* which the observer sees through the opening; *A* 1 is considerably darker.[2] The distance at

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[1] The experiments were carried out in the Psychological Institute of the University of Berlin, and in the rooms of the 'Graduate Faculty' of 'The New School for Social Research' in New York. I wish, at this point, to express my gratitude to Professor Max Wertheimer for his interest in my work.

[2] I shall not, either here or later, give quantitative results of the experiments. All statements on differences in brightness and color were experimentally measured (the method used was that of equating the respective colors with a color wheel). It seems, however, unnecessary to publish the tables since these experiments are not concerned with the extent of the differences, but only with their direction. And moreover I am using only those results where the direction of differences was established without exception by each of the (at least 5) observers.

which the observer sits has been so chosen that he does not notice that the screen is bent. He furthermore does not perceive a shadow on the right side, but sees only that the left side is brighter and the right side is darker. The observer therefore gets the impression of a unified illumination. In this situation there is scarcely any difference between the brightness of *A* 1 when observed through a reduction screen or when observed without it.[3] In the centre of *A* 1 the observer sees a bright spot. As a

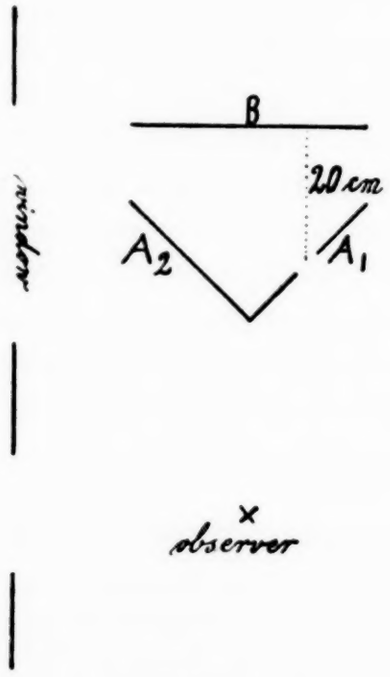

FIG. 1

rule he does not see a hole, but rather a spot on the surface of the big dark screen in front (*A* 1). The dark color surrounding the spot induces a brightening contrast effect, causing the spot to look brighter.

In the second step of the experiment the observer is sitting at a distance of 1½ meter. In this case, as a rule, he notices that the screen is bent, and he sees on the left side an illuminated medium grey and on the right side a shadowed medium grey. Now the entire perception has changed. At this time there is a differentiation of the illumination and there is a considerable brightness constancy: In step 2, if *A* 1 is observed through a reduction screen, it appears very much darker than without it. However, once again, a spot is seen

[3] Katz introduced the method of a reduction screen (that is observation through a small hole in a medium grey screen, allowing only a protion of the object to be seen) as the easiest means of establishing whether the phenomenon of color constancy existed or not. ('Reduction' is described by Macleod—*Arch. Psychol.*, 1932, 135—who also explains at length most of the terms used in this paper.)

in the surface of the front screen and not a hole. The brightness of the spot has changed, compared with its brightness in the first step of the experiment. The spot has become brighter. It now appears even brighter than screen *A* 2. If the observer moves slowly toward the screen, starting from a distance of 3 meters, he will observe a gradual phenomenal brightening of the spot.

The same experiment can be performed in a slightly altered form. In this case it is expedient to use experienced observers. Screen *A* now has the form shown in Fig. 2. It is

FIG. 2

easy to *invert* this form. The observer—at a constant distance of $1\frac{1}{2}$ meter—may see '*a*' in front and the sides '*b*' and '*c*' in the back (as it is objectively the case), but he may by a subjective inversion push '*a*' toward the back and '*b*' and '*c*' toward the front, or he may also see '*b*—*a*—*c*' as approximately one level surface. In the case of the latter attitude we find the same conditions as in the first step of the former experiment. The shadowed right side appears dark and the left side bright; the observers do not have the impression of a shadow, but see two differently colored surfaces. When the observer's attitude corresponds to the real position of the screen ('*a*' in front), then there appears on the right a shadowed and on the left an illuminated medium grey in the same way as in the second step of the experiment. Having this attitude the spot appears considerably brighter than when the screen is inverted.

The possibility of changing the distance experiment into an inversion experiment is of importance, because it eliminates the possible objection that the brighter appearance of the spot when observed from a closer distance is due to the increase of the angle of vision.

To explain the fact that the spot appears brighter in some

of our experiments than in others the following factors must be taken in consideration:

(*a*) *Effects of Peripheral Light Reflection.*—In this respect there is, however, no difference between the two steps of our experiments. The light reflection both of the surrounding field (*A* 1) and of the spot (*B*) remained the same. Therefore it is not possible to state that the contrast (the brightening influence exercised by the light of the surrounding field on the infield) was greater in the second than in the first step of our experiments.[4]

(*b*) *Effects of Color Phenomena.*—In the second step of the experiment screen *A* 1 appears brighter than in the first step. But if it were the phenomenal color of the screen which had had an effect on the contrast induction (which would be contrary to statements of Katz with which we shall deal later on) the results of our experiments should have been opposite to what they were. The brighter appearing field *A* 1 in the second step should have induced a lesser brightening contrast; that is, the spot should have appeared, in this case, darker than in the first step of the experiment. Actually, however, both the surrounding field and the spot became brighter. Therefore, if there was any effect of color phenomena on the degree of contrast, it must have been overcompensated by some other factors.

(*c*) *Effects of the Perceived Illumination.*—In the first step of our experiment and also in the case of the inversion of the screen there is a unified illumination, that of the whole room. In the second step of the experiment, however, the entire right side of the screen including the spot lies in a shadow. The results of the experiments seem to prove that the reduced illumination on this side has a brightening effect both on field *A* 1 and on the spot. Such effects of changed illuminations are usually called constancy effects. Therefore one might state the result of our experiments so far by saying that in the first step only contrast phenomena occurred, while in the second step both contrast and constancy phenomena were present. In the second step brightening effects of the illumination (constancy) were added to the brightening effects of the surrounding field (contrast). In this case constancy seems to increase the contrast, both having the same direction.

§ 2. "*Constancy Decreases the Contrast*"

The second experiment differs from the first one in so far as screen *A* in front is white and not medium-grey. Furthermore, the angle at which the screen is bent is now increased by half (angle-135°). Therefore there is only a slight shadow on the right side of the screen. According to the measurements of light reflections, screen *A* 2, the illuminated white, is now the brightest, then follows *A* 1, the shadowed white, and the darkest is *B*, the unchanged grey screen in the back. In the first step of the experiment—the observers sit $3\frac{1}{2}$ meters away

[4] Previous investigations have moreover proven that the contrast grows with the increase and not with the decrease of the distance of the observer from the object. These changes of contrast effects are considerably smaller than those noticed in our experiments. (See *Katona, Zeits. f. Sinnesphysiol.* **53**, 173.)

—the judgments of the observers concerning the perceived brightness are the same. They see on the left a white and on the right a light grey surface while the spot in the centre of the right screen is considerably darker than the surrounding grey. Contrast, here, also serves to increase the darkness. The observers get the same color impressions if they observe the screen in the attitude of subjective inversion at a distance of $1\frac{1}{2}$ meters.

In the second step of this experiment, sitting at a distance of $1\frac{1}{2}$ meters, the observer perceives on the left an illuminated white and on the right a shadowed white. *A* 1 appears considerably brighter than in the first step. The spot becomes brighter too. In the first step it appeared nearly black while now it appears middle-grey. As one comes nearer one can observe the phenomenon of brightening.

This result can again be explained only in the same way as the result of the first experiment. Light reflection or the phenomenal color of the surrounding screen (*A* 1) cannot be considered responsible for the brightening effect of the spot. But the illumination of the entire right side became lesser in the second step, and therefore a constancy effect, both for *A* 1 and for the spot, may be assumed. This effect has, in the second experiment as well as in the first one, the direction of increasing the brightness. The contrast effect has, however, in the second experiment the direction of darkening, because the spot is darker than the surrounding field. The result of the second experiment might therefore be stated as follows: contrast is reduced by constancy, constancy is opposite in direction to contrast.

§ 3. *The Functional Difference between Contrast and Constancy*

In order to clear up the apparent contradiction between the results of the two experiments we will attempt to set up schematic diagrams demonstrating how the contrast and how the constancy function. Contrast is usually understood, according to Hering, as the interaction of neighboring visual areas. It is either the reciprocal repulsion of two neighboring areas or else it is the repulsion of the infield by the surround-

ing field. The latter is the case if a small infield is surrounded by a larger field, and there is no contrast effect apparent in the perception of the surrounding field. In Fig. 3, which gives the diagram of such behavior of contrast, two small papers are lying on a larger paper, one infield being brighter and one darker than the surrounding field.

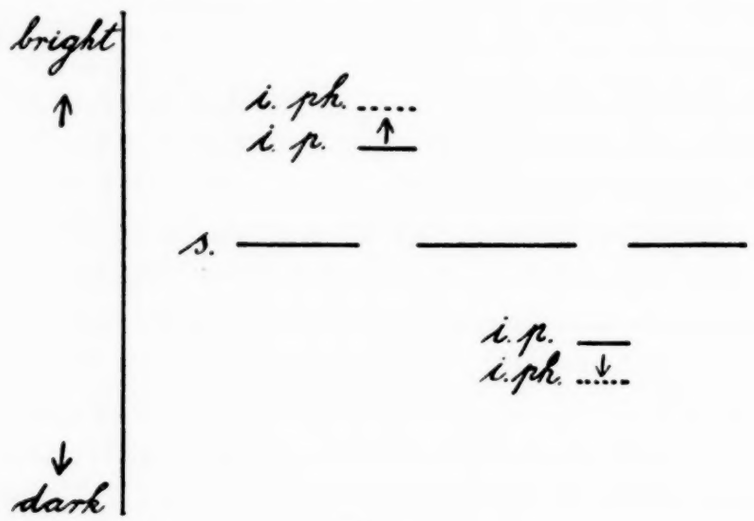

FIG. 3

s.—surrounding field p.—peripher
i.—infield ph.—phenomenal
Upward indicates brighter, downward darker.

A diagram which would illustrate the effect and direction of constancy phenomena would be very similar to the diagram of contrast. We usually assume that there is constancy if a shadowed grey paper (*e.g. A* 1 of Fig. 1) does not appear as dark as it ought to appear according to the conditions of physical light reflection, or a well illuminated grey paper does not appear as bright as it ought to. A special illumination seems therefore to have a contrary effect—a brightening effect if a shadow, and a darkening effect if a special light is apparent. (A diagram showing these effects may be found in Fig. 5, overlooking, however, the lines marked with a star, which will be explained later.)

If these considerations were correct, then contrast and constancy would be very closely related phenomena. In that case by changing the term surrounding field to the term special illumination contrast would be changed to constancy. That

is, if a small light grey paper is surrounded by a large dark grey paper the brightening of the light grey is called contrast, and if the light grey paper is lying in reduced illumination its brightening is called constancy.

According to the findings in our experiments the actual situation seems to be less simple. The results of our first experiment correspond with the diagrams. For the sake of simplicity let us overlook the left side (*A* 2) of the experimental set-up. We have therefore one surrounding field (*A* 1) and one infield (*B*), which in the first experiment—observation from a distance of $3\frac{1}{2}$ meters—behave as shown in the left side of Fig. 3: the infield becomes brighter owing to the influence of the darker surrounding field. In the second experimental step there exists, contrary to the first step, a special illumination which has a brightening effect both on the surrounding field and on the infield. Fig. 4a intends to demonstrate that the

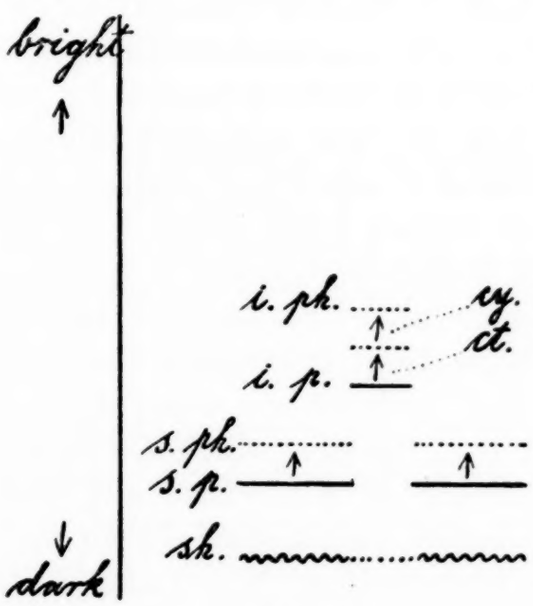

FIG. 4a

ct.—contrast
cy.—constancy

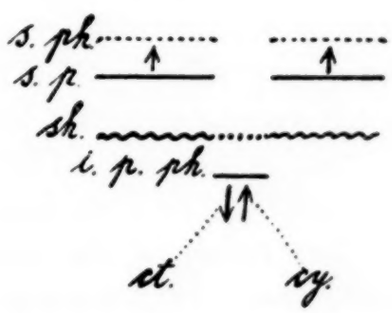

Fig. 4b

sh.—shadow

(The diagrams illustrate only the direction but not the extent of the changes. Concerning the extent, but not the direction, there are considerable individual differences.)

brightness of the infield is increased in this case twofold, by contrast and by constancy.

To draw a diagram of the first step of the second experi-

ment is again easy. The surrounding field, a shadowed white appearing light grey, is brighter than the infield and exercises a darkening influence (as shown on the right side of Fig. 3). In the second step a special illumination is apparent and must be noted in the diagram. The shadow is a slight one since the sheet is greatly bent. Were the shadow stronger, the surrounding field would reflect less light, and it would exercise a brightening contrast induction on the spot which is objectively free from the shadow. But as already proven by the first step, the contrast induction brings about a darkening. Therefore in the diagram of the second step the shadow has to be indicated at a relatively high position, higher than that which corresponds to the brightness of the infield (see Fig. 4*b*). The effect of this special illumination (shadow) on the surrounding field (*A* 1) is a brightening one as may be concluded from the diagram and from the results of the experiments. Now the infield also appears brighter than in the first step. However, it must be assumed that again a darkening contrast effect took place. But this effect must have been compensated by the constancy effect. The fact that the spot appeared brighter in the second step can be understood only if one assumes that in this step a constancy effect existed which made both the spot and the surrounding field appear brighter.

The result of the second experiment makes necessary to supplement our previous considerations concerning the function of constancy. At that time we have not taken into account the possibility that brightness grades which are darker than a shadow may be influenced by a shadow, and that brightness grades which are brighter than a special light may be influenced by that light. The complete Fig. 5 also deals with such cases and demonstrates the effects according to the results of our experiments. A shadow *always* has a brightening effect both on brighter and on darker colors, whereas a special light always has a darkening effect.

In this sense one may speak of a unilateral effect of constancy. And thereby an important difference is established between constancy and contrast, namely: whereas the effect of constancy is unilateral, the effect of contrast, according to our

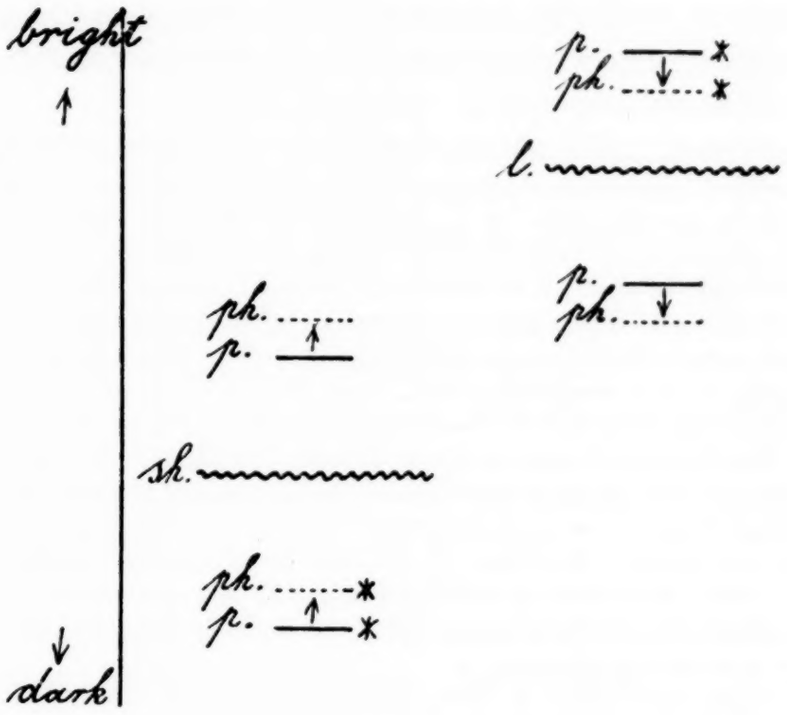

FIG. 5

sh.—shadow l.—light

~~—indicates special illumination.

present knowledge, is reciprocal.[5] (The contrast effects are reciprocal, because a small light paper placed on a large medium grey becomes brighter while a small dark paper on the same medium grey becomes darker.)

§ 4. *The Direction of Constancy Effects*

Our findings up to the present do not yet make clear what kind of special illumination has a brightening effect and what kind has a darkening effect. It might be possible that there exists an absolute borderline between shadow and light. Then every illumination below the line would have a brightening effect, and every illumination above it would have a darkening effect, while in a 'normal illumination' no con-

[5] This question of principle can, of course, not be established by the result of one experiment. However, in this publication I have the intention only to demonstrate the possibility of such a distinction between contrast and constancy, and therefore I delay the publication of further experiments in a different field, which seem to point to the same explanation. I wish to add that the literature of color constancy, to the best of my knowledge, does not refer to any experiments which would contradict the above statement.

stancy effects would be apparent. However, according to our second experiment with its bright shadow it is improbable that this hypothesis is correct.

Several previous investigations have dealt with the question of the direction of constancy effects. The traditional answer is that the 'transformation' goes in the direction of the genuine color. It is admitted, however, that there are constancy effects with objects of unknown color, and thus the known genuine color of an object cannot determine the direction of the effect. Therefore the genuine color is defined as the appearance or the alleged appearance in normal illumination. Katz [6] has proven that normal illumination—clear daylight without direct sunshine securing good recognition—is a phenomenal fact. Manyfold observations and experiments seem to confirm the thesis that the transformation is directed towards the appearance in normal illumination. A short time ago *Thouless* [7] declined to use the term constancy, because it does not fit several included phenomena, and he proposed to replace it in accordance with the just mentioned thesis with the expression 'phenomenal regression to the real object.' According to Thouless the phenomenal character of the object is a compromise between the 'real' character and the character given by peripheral stimulation, the 'regression' having the direction away from the stimulus character toward the real character.

I have made experiments [8] in which a small grey disc and its neighborhood were illuminated by the rays of a strong projection lamp and, besides, by the normal illumination prevailing in the whole room. Observed through a reduction screen, the disc appeared considerably brighter than without the screen. The constancy phenomenon could be increased in different ways: the more it increased, the more the disc was similar to its appearance in normal illumination. This experiment which seems to confirm the previous thesis could, however, be reversed. Were the disc and its neighborhood seen in bright daylight, that is in normal illumination, but the entire visual field (the wall and the objects before it) illuminated in addition by a strong projection lamp, then again a difference in the brightness of the disc was found, whether it was observed through a reduction screen or without it. If the constancy effect was very large then the disc appeared as it would have appeared on the wall, that is abnormally illuminated. In this experiment the constancy had a direction towards the appearance in abnormal illumination. However, if we use instead of the term normal illumination, the term prevailing illumination, there is no contradiction between the results of the two experiments. In both cases the constancy goes in the direction of the appearance in the prevailing illumination. In the first case the prevailing illumination was accidentally identical with the normal illumination while in the second it was not. It is important to distinguish between normal and prevailing illumination since the term prevailing illumination does not involve the idea that past experience, knowledge, etc., are required in order to determine the direction of constancy.[9]

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[6] *Die Erscheinungsweise der Farben*, Leipzig, 1911.

[7] *Brit. J. Psychol.*, 1931/32, **21/22**, and *Psychol. Forsch.*, 1934, **19**.

[8] *Psychol. Forsch.*, 1929, **12**, 107–8.

[9] A. Gelb (Die Farbenkonstanz der Sehdinge, in *Handbuch der Physiologie*, edited by Bethe, Vol. XII/1, 1929), relying on another experiment, (p. 653) has proven that color vision in normal illumination is not of specific significance in the development of our color vision.

The function of constancy could therefore be stated by saying that a color which is abnormally illuminated appears changed in the direction of its alleged appearance in the prevailing illumination. According to this conception it is possible that a genuine color or a color in normal illumination might be 'transformed.' Furthermore, a color seen through a hole might be transformed if it appears as a part of the screen containing the hole. Finally, it seems possible that not an actual special illumination but only the impression of such an illumination is a requisite. The following experiment confirms this. I succeeded in bringing about constancy having a paradoxical direction, which was the effect of an abnormal illumination which actually did not exist.

The following experimental set-up was used: the largest part of the visual field consisted of a wall covered by paper, unsaturated light green in color. Set in place, in the centre of the field was a glass plate, 6 × 9 centimeter, of the same unsaturated light green color, which under normal conditions was always promptly designated as green. Around the plate several different objects all painted with the *same* color were fastened in irregular order: playing cards, a drawing of a face, a small box. The observer sat behind a screen which was lifted up for a few seconds for each of the experiments. At first constancy experiments were made with an other set-up which was placed in front of the 'green set-up.' In addition to the normal illumination, not too bright daylight coming in from the side, which is always present in these experiments, I used various colored lamps. After a few experiments with those lamps, which were done as a frame work, the green set-up was shown, always *only* normally illuminated. The observer was asked to state the color of the plate in the centre, in the same way as he did with other plates in the experiments immediately preceding. (In group experiments the observers had to write down the color of the plates.) The answer given by a large number of inexperienced observers was: grey, dirty-colorless. Only a few observers used also the expression 'greenish.' Most of the observers asserted afterwards that they had not seen any chromatic hue. (Similar

were the results with experiments made with a set-up of a more saturated green. Then answers were given as 'greenish,' 'pale green.') When asked later what color illumination had been used in the experiment many of the observers answered 'green.' They had an impression of an artificial green illumination which covered the entire wall.[10]

This experiment forms another argument against the conception that constancy assists the appearance of the genuine color. It would be possible to state the results by saying that constancy has the direction toward the alleged color. However, in constancy effects there are no operations of thinking involved, and no phenomenal changes of color occur, but a color appears, automatically, and can be judged by later comparison to be different from the expected effects of the stimuli. Therefore it is very unsatisfactory to assume a functional effect of an alleged color. The observers do not think that the green plate is really grey, but they see a grey plate. This difficulty can be overcome, and our experiment explained, by assuming that it is not the genuine color and not the alleged appearance of a color which determines the direction of constancy, but the impression of the special illumination. In the last mentioned experiments the observers get the impression of an abnormal green illumination. Simultaneously with this impression all colors appear as though devoid of green. In our experiments with the bent screens the impression of a shadow on the right side arises; simultaneously the colors in the shadow appear brighter than the stimuli would warrant.

The direction of constancy is therefore away from the abnormal illumination. The impression of an abnormal illumination is derived by differentiation from the prevailing illumination. The abnormal illumination can therefore not be determined absolutely but only in contrast to the prevailing illumination level. It is the relation of these two which determines the direction of constancy effects. To summarize:

[10] The impression 'grey' for the object was, however, more frequent than the impression 'greenish' for the illumination. This is to be attributed to the fact that the previous 'frame' experiments dealt only with the object color and not with the illumination.

A color seen in an abnormal illumination appears changed in the direction which is contrary to the direction in which the abnormal illumination differs from the prevailing illumination.

A shadow is a change from the prevailing illumination in the direction 'darker' (there exist light shadows, but they must be darker than the prevailing illumination). All object colors, whether light or dark, appearing in the shadow, appear brighter than would correspond to their stimulus character. The same is true of special lights which influence all object colors in the direction of darkening. The difficulty involved in the question, which illumination has a brightening effect and which a darkening one is herewith solved. The answer is always found in the difference between the impression of the abnormal and the prevailing illumination, that is from the entire actual perception.

This description of brightness constancy applies to color constancy as well. It is not sufficient to have an artificial illumination in order to get constancy phenomena, but the impression of a colored special illumination must occur in contrast to the illumination level of the entire visual space experienced at the same time. Such an impression is sufficient to have constancy effects, even without actual special illumination as proven by our 'green-experiment.' The direction of the constancy effects is contrary to the differentiation of the special from the prevailing illumination.

* * *

Since it is the difference between the special illumination and the prevailing general level of illumination which is decisive for constancy phenomena, the phenomena can be increased by indications present somewhere within the perception, even if there is no direct connection between the indication and the color on which is brought to bear the constancy effect. This writer made an extensive study to find out the factors which further constancy (*Psychol. Forsch.*, vol. 12) and introduced in 1929 the term supports of constancy ('Stuetzen der Konstanz') to designate those factors in the visual space which help the observer to perceive a special illumination and thereby to increase constancy effects.[11]

By trying to establish a system of the cues to constancy I have found that constancy effects are mainly furthered by enrichment of the perception through better organization, more contours, more form and object-characters, movement, etc. In this way we may arrive at another distinction between contrast and constancy, since contrast is furthered in general by a better coherence of infield and surrounding field, that is by a sort of homogeneity of the perception. (I do not want to deal here with the problems of increasing contrast, but refer only to the facts that larger contrast effects are obtained when rotating the color wheel or placing a tissue paper on both the infield and the surrounding field, etc.) From similar observations *Thouless* concluded that "the conditions in which phenomenal color induction is diminished are also those in which phenomenal regression is more effective in other experiments" (Vol. 22, p. 13). He believes that contrast phenomena are comparatively infrequent, because they are reduced by phenomenal 'regression.' However, it is not so that contrast and constancy always have an opposite direction (I wish only to recall to the first experiment of this paper), but it is true that the fundamental conditions for the occurrence of

[11] *Thouless* introduced in 1931 the term 'perceptual cues to the real character of the object' (Vol. 22, p. 7), but speaks on a later occasion also about 'cues to the differences in illumination of the two papers' which is more in our sense.

contrast and constancy are different. The constancy phenomena are closely connected with the fundamental questions of perceptions, the ways of the organization of our visual space.

§ 5. *The contrast-constancy experiment of Katz*

Katz has formulated the results of his first constancy experiments (carried out 1907) as follows: The phenomenal color of the surrounding field has no influence on the contrast induction, the latter being solely dependent on the quality and intensity of the retinal excitations.[12] We have already referred to this thesis when considering the explanation '*b*' of our first experiment. The method used by Katz in his experiments was to place one small grey disc in front of a shadowed white background and an other identical grey disc in front of an illuminated black background. The backgrounds appear different when observed in a normal way, but equal (or equally bright) when observed through a reduction screen. The two small discs appear equal even when observed without a screen: The contrast induction of the two differently appearing backgrounds must be the same. Wolff (*Psychologische Forschung*, Vol. 18, 1933) recently tried to show that the experiments of Katz do not warrant his conclusions. The two small discs are localized either within or outside the two differently illuminated spaces. If they are localized within them, then they do not appear equal owing to constancy effects which they undergo. If they are localized outside of them, then they may appear equal, but surrounding field and infield are seen separated from each other in a way which practically excludes contrast induction.

In the experiments which I carried out, similar to those of Katz (who does not describe his experimental set-up in detail), I tried to find a compromise. In order to reach this compromise, the discs should not be hung on thin threads (because then they are localized within the differently illuminated spaces), nor placed on stands far in front of the illuminated spaces (because then they are localized outside of the spaces and then scarcely any contrast occurs, as Wolff found by using stands). In my experiments the discs were pasted on small wooden pegs, which stuck out from the partition separating the two differently illuminated spaces.

The left side of screen *A* (*A* 1) is covered by a black paper directly illuminated by the window on the side. The right side (*A* 2) is covered by a light grey paper which is entirely in the shadow of the partition. Both papers (15 × 25 centimeter) appear equally bright if observed through the hole of a reduction screen. The depth of the partition is 11 cm. On both sides of the partition, near the front, two small sticks (similar to tooth picks) are fastened; on their end small medium grey paper discs, 1 × 1½ cm, are pasted (*D* 1 and *D* 2). The sticks are turned slightly toward the observer, and therefore the two small discs are equally illuminated. They also appear equal if a large grey surface is placed directly behind them. The distance between screen *A* and the small discs, that is between surrounding fields and infields, is 12 cm, that between the discs and the observer is 1½–1¾ meter. The observer sees (without the use of a reduction screen) *A* 1 black, *A* 2 considerably brighter, but both discs equal.

Whether this experiment proves the point, must be found out through further experiments:

1. Screen *A* is removed. An evenly illuminated wall (20 centimeter behind screen *A*) now constitutes the background of the discs. The discs appear equal.

2. Screen *A* is fixed on the wall. The backgrounds are therefore black and light

[12] The experiment is again described in *Aufbau der Farbwelt*, Leipzig, 1930, p. 393.

grey papers equally illuminated. Now *D* 1 appears considerably brighter than *D* 2. This observation proves that contrast effects can take place in the given space conditions, but the results are not yet conclusive since the difference between the two surrounding fields is now extremely large.

3. A paper whose brightness lies between a medium and a dark grey is chosen which, if normally illuminated, appears to have the same brightness grade as the shadowed light grey paper (*A* 2) in the original set-up. This new grey, fixed on the wall, serves as normally illuminated background of *D* 2. The original black paper, also on the wall, serves as background of *D* 1. The result of this experiment is: *D* 1 is brighter than *D* 2, but the difference is very small.

If experiment 3 is compared with my original experiment, the thesis of Katz is confirmed. Retinally equal but phenomenally unequal backgrounds do not induce different contrast effects, although the same phenomenal difference, if brought about by two backgrounds of different light reflection, does induce different contrast effects.

In spite of this confirmation one must be very cautious concerning the conclusions. The brightness difference between *D* 1 and *D* 2 in experiment 3 was very small, which means that the contrast effect was in the original experiment very small too. Moreover, also the constancy effect was very small. Although I intentionally have chosen paper surfaces as backgrounds, and no rotating color wheels, because I did not want to exclude the perception of the microstructure of the papers and wanted thereby to provide a cue to the constancy effects, the shadowed bright grey paper nevertheless appeared dark grey. The constancy effect was small, because the shadowed space was not articulated, and the small paper discs *D* 1 and *D* 2, that is parts of the perception not in shadow, constituted the center of the perception (the main point of attention).

The results of this experiment are not in contradiction to the assumption which we have used to explain the experiments with the bent screens, that is that constancy phenomena might under certain circumstancse influence contrast phenomena. In the case of Katz's experiment, artificial methods were used to eliminate the influence of constancy on contrast: In the conditions in which one may reach Katz's results, the contrast phenomena occur outside of those parts of perception which are affected by a special illumination. In the experiments with the bent screens, on the other hand, there is an effect of constancy on contrast, because the contrast phenomena occur in those parts of the visual space which are affected by the difference between the special and the prevailing illumination.

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THE RELATION OF GALVANIC SKIN REACTIONS TO PRECEDING RESISTANCE*

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I. Introduction

In a recent study (7) the writers investigated the effect of repeating an electric shock on certain reactions of human subjects. The reactions included galvanic skin reflex[1] and recovery, respiration, muscle thickening, and gross bodily movement. For details of technique and results the reader is referred to the previous monograph. A brief resumé will suffice to introduce the problem of the present paper.

Twelve subjects, six men and six women, were given five shocks daily for approximately four weeks in some cases and eight weeks in others. Changes in apparent resistance[2] to direct current were measured by a Wheatstone bridge with three arms constant throughout. The gsr was computed as an ohmic drop and expressed as a fraction of the resistance just preceding stimulation. Recovery was determined by the maximum resistance reached during one minute following the stimulus, and the ohmic rise was expressed as a ratio to the preceding ohmic drop.

As would be expected, there was a general tendency for reactions to diminish from day to day. This was by no means so marked or so uniform in the galvanic as in the muscular responses. In attempting to explain the discrepancy, we soon felt the need of going back of the fractional measures of galvanic reactions to the absolute resistances preceding them. To be more specific, it might be argued that the size of the reflex is partially dependent on the course of preceding re-

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[1] Hereafter referred to as *gsr*.

[2] 'Resistance' will hereafter be used to signify the subject's total apparent rather than true ohmic resistance.

sistance changes. Cattell (1) presented evidence that the reflex following a rising resistance curve tended to be greater than that after the more frequent falling curve. He also held that a rising curve might indicate a lowering of excitement. The relatively slight decrease of the galvanic reflex with repetition, then, might be due to a tendency of the preceding resistance to remain at a higher and higher level as the subject's apprehension declined. This hypothesis could be verified only by an analysis of the daily resistance measurements preceding stimulation.

Besides the course of reactivity from day to day, changes from shock to shock on the same day were computed. Here our measure of gsr showed a gradual fall closely parallel to the curves of respiration and muscle thickening, while recovery showed a rise. The factors determining the frequently reported "fatigue" of the reflex are in need of clarification. Recovery has been chiefly investigated by Darrow (2). The present data offered an opportunity for analysis which might throw some further light on both reflex and recovery.

II. Results

1. *Resistance Changes Within a Given Day*

A single experimental session comprised a rest period of three minutes with the current turned on, followed by five shocks separated from one another by approximately one minute. Measures of resistance were secured at the following times: (1) As soon as the bridge was balanced after the current had been turned on. This interval was not timed and undoubtedly varied somewhat from day to day, but probably averaged about 15 seconds. The length of time the subject's fingers had been immersed in the salt solution of the electrodes before the circuit was closed also varied, and much more widely, with a probable central tendency of about one minute. (2) Just preceding each shock. (3) At the minimum point of each gsr to a shock. (4) At the maximum point reached during the following minute.

The corresponding resistances on successive days were averaged for each subject. Table I presents these data with

TABLE I

AVERAGE RESISTANCES IN OHMS AT SPECIFIED POINTS DURING THE EXPERIMENTAL PERIOD

Subject	N	Initial	Shock I			Shock II			Shock III			Shock IV			Shock V		
			Pre.	Min.	Max.	Pre.	Min.	Max.	Pre.	Min.	Max.	Pre.	Min.	Max.	Pre.	Min.	Max.
Zuzu.....	29	14184	12290	10511	11700	11569	10566	11844	11668	10609	11748	11281	10288	11428	11097	10047	11235
Wek.....	29	13051	10523	9024	9637	9503	8394	9033	8988	8077	8773	8686	7952	8573	8474	7801	8348
Tif.....	29	12488	10640	9887	10588	10300	9688	10305	10111	9572	10147	9953	9437	9974	9878	9316	9821
Gib.....	27	13331	11347	10251	10996	10824	9864	10531	10384	9538	10206	10001	9167	9782	9649	8873	9435
Dar.....	26	11419	10058	9313	9963	9690	9095	9759	9446	8838	9487	9112	8554	9128	8876	8315	8914
Hel.....	24	8238	7383	7079	7397	7199	7016	7285	6914	6740	7018	6681	6545	6883	6557	6426	6816
Den.....	24	12238	10328	9509	10220	9629	9047	9754	9167	8781	9519	9011	8626	9316	8863	8475	9094
Lab.....	23	8628	7215	6451	6995	6868	6287	6959	6736	6245	6841	6616	6103	6632	6377	5882	6408
Les.....	54	11737	10483	9932	10377	10131	9638	10041	9792	9401	9767	9570	9222	9572	9416	9103	9456
Pom.....	43	12997	12444	11539	12078	11842	11201	11715	11417	10907	11423	11123	10652	11139	10884	10434	10929
Yix.....	55	11011	8652	8356	8774	8383	8120	8565	8184	7929	8393	8013	7754	8217	7915	7623	8158
Bok.....	54	10741	8464	7846	8482	7992	7542	8135	7709	7267	7809	7383	6973	7480	7119	6730	7206
Av.....		11672	9986	9142	9767	9494	8872	9494	9209	8658	9261	8952	8440	9010	8759	8252	8818

readings arranged in chronological order. The *N* column contains the number of days entering into each subject's averages, only those days being included with records clear of technical defects.

The averages at the foot of the table are graphically depicted in Fig. 1. The average resistances in ohms just

FIG. 1. Average resistances in ohms at specified points during the experimental period.

preceding each shock, at the minimum of the reflex, and at the maximum point thereafter are represented by the columns as indicated. The stippled column stands for the initial reading at the opening of the rest period. The interval between the first two columns denotes a lapse of three minutes; the intervals between the black columns, one minute each.

A glance will suffice to show that all three columns decrease progressively from shock to shock. A curve drawn through the tops of the stippled and black columns would show a smooth descent with slight negative acceleration. Although the individual data of Table I are not graphically presented, such curves have been drawn by the authors and show the same general characteristics in each case. Even these curves are averages of a number of days' records, yet they represent the data for any given day with striking fidelity. The reading at three minutes is consistently lower than the initial reading; there are only 20 exceptions in the entire 417 daily records, and 12 of these occur among the 43 records of a single subject, Pom. A general decline in resistance from beginning to end of the experimental period is the rule.

While all three columns in Figure 1 decrease in height from shock to shock, a closer inspection shows that their respective

decreases are not strictly parallel. The difference between preceding and minimum resistances, for example, is greater at the first shock than the fifth. The maximum, while lower than the preceding at the first shock, is higher at the last three. These comparisons point to variations in the amounts of reflex and recovery from shock to shock.

Figure 2 brings out this variation more clearly. This

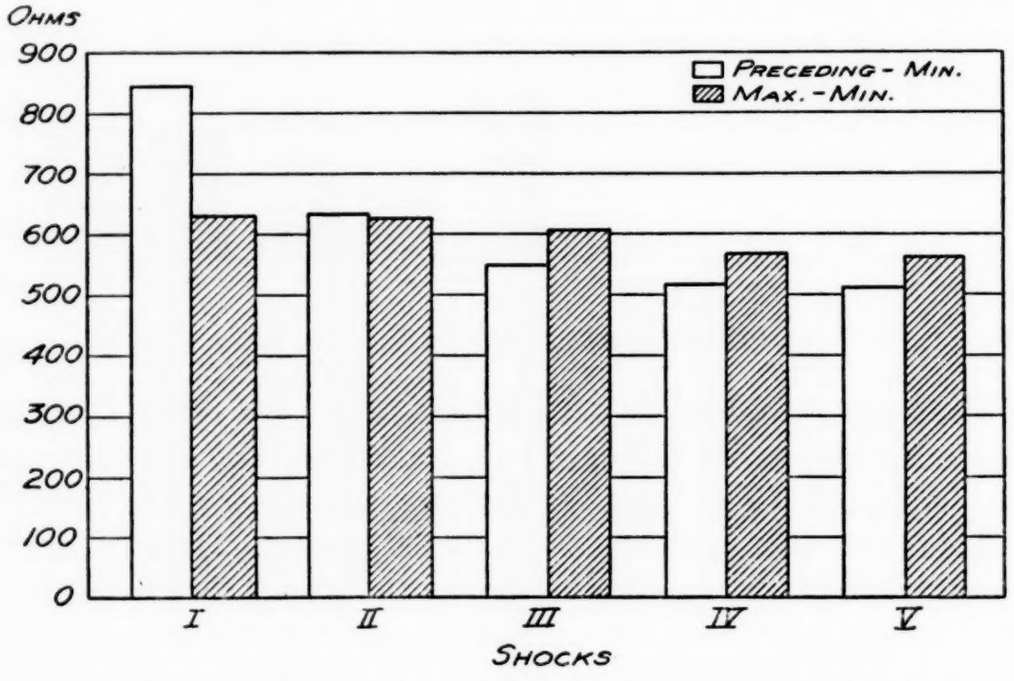

FIG. 2. Average resistance drop and recovery in ohms for each shock.

figure was also derived from the lowest line of Table I. Each white column stands for the difference between the corresponding preceding and minimum resistances; *i.e.*, the average amount of the gsr expressed in ohms. The cross-hatched column stands for the difference between maximum and minimum resistances; *i.e.*. the average ohmic recovery. While the white columns show a smooth, negatively accelerated decrease in the absolute size of the gsr, the recovery columns show only a slight linear descent. This provides the basis for our previous finding that recovery relative to the preceding reflex increased from shock to shock. With minor variations the data of the individual subjects show the same characteristics.

Results reported by Darrow (**2**) raise the question whether

our data supply grounds for predicting a negative correlation between minimum resistance and relative recovery. Darrow measured the galvanic reactions of 90 subjects to a series of 18 varied stimuli. Expressing the amount of recovery in three seconds as a percentage of the minimum resistance, he found consistently high negative correlations between recovery and minimum resistance itself. In our data relative recovery, to be negatively correlated with a declining minimum resistance, would have to increase from shock to shock. Actually ohmic recovery yields the following percentages of minimum resistance for the five shocks in order: 6.8, 7.0, 7.0, 6.8, 6.9. Our failure to confirm Darrow's finding may be due to our measuring recovery in one minute instead of three seconds. This is especially plausible in view of the negative correlation he also found between minimum resistance and *rate* of recovery. It would be interesting to know whether his subjects' resistance levels were also declining during the experimental period, as in the present study.

2. *Resistance Changes from Day to Day*

As noted above, a resistance reading immediately preceding the first shock was taken approximately three minutes after the current had been turned on. Changes in this reading in the course of daily trials may be followed in Table II, which presents averages for days 1–5, 10–14, and 20–24. Examina-

TABLE II

AVERAGE RESISTANCES IN OHMS AFTER THREE MINUTES

Subject	Day 1–5	Day 10–14	Day 20–24
Zuzu	12838	12568	11806
Wek	10645	11113	10698
Tif	9360	10436	10212
Gib	10855	11927	11825
Dar	9422	10905	9452
Hel	6860	7376	7414
Den	11312	10112	9823
Lab	8000	7036	6142
Les	12994	9226	11948
Pom	13159	11420	14260
Yix	9804	8508	8964
Bok	10010	9428	9766
Av	10438.2	10004.6	10092.5

tion of the individual data gives no evidence of consistent trends from subject to subject. Moreover, the averages of the subjects' averages approximate a horizontal line.

In apparent opposition to these findings are data reported by Davis (3) from experiments on the effect of daily repetition of a loud sound. Curves representing average resistance readings taken one minute after the onset of the current show a decided rise over a five-day period. For more direct comparison we have tabulated resistance readings after three minutes for each of the first five days and present them in Table III. The averages are plotted in Fig. 3. Disregarding

TABLE III

RESISTANCES IN OHMS AFTER THREE MINUTES

Subject	Days				
	1	2	3	4	5
Zuzu.....	14580	12860	13470	——	10440
Wek.....	——	11380	10850	10150	10200
Tif.....	10250	10350	8050	8410	9740
Gib.....	——	10640	10310	9220	13250
Dar.....	9100	11360	8200	8510	9940
Hel.....	——	——	7110	6880	6590
Den.....	12900	10810	10740	9540	12570
Lab.....	9010	6780	7300	8230	8680
Les.....	15330	14680	11440	12260	11260
Pom.....	9610	14000	13820	14580	13785
Yix.....	9170	9920	10310	10210	9410
Bok.....	9910	11440	10260	9240	9200
Av.....	11124.0	11292.7	10155.0	9748.2	10422.1

differences in absolute resistance level, the most striking difference is the direction of the curves. While Davis's curve shows a 40 per cent rise, ours shows a drop, which, however, amounts to only 12 per cent and may well be due to chance. This discrepancy may be due to different degrees of habituation to the stimuli used. The way in which this factor may operate to bring about a rise in resistance is suggested by an inspection of curves presented by Davis, which show the course of resistance change within a trial. Each succeeding day's curve starts at a higher point at one minute and falls to a more uniform level at six minutes. This may mean that on the first day the resistance had dropped so steeply that by

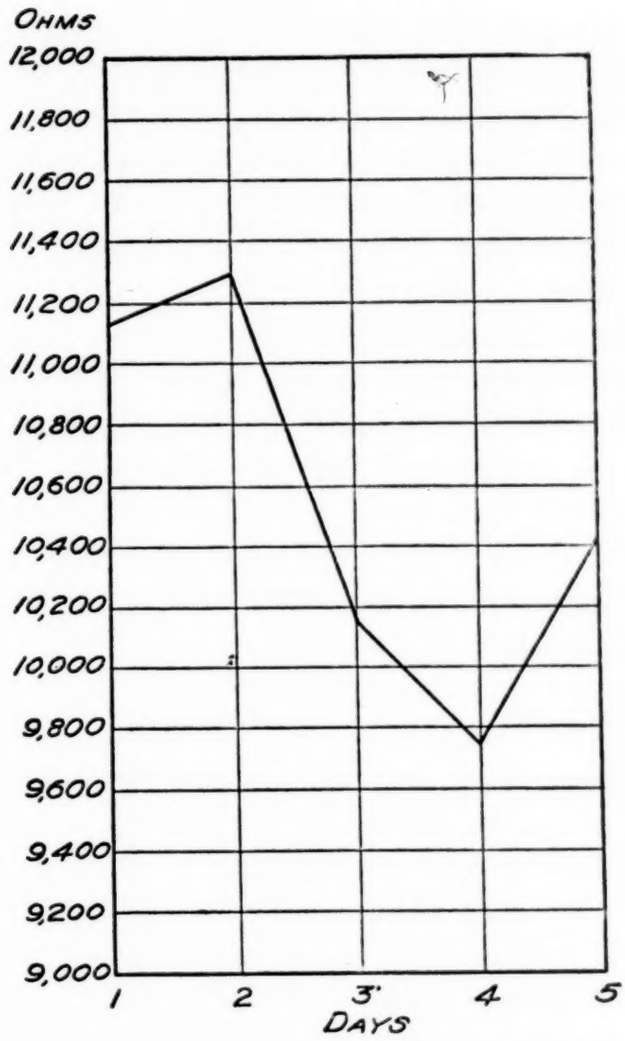

FIG. 3. Average resistances in ohms after three minutes.

the time the initial reading was taken it was already close to physiological equilibrium. Decrease of tension on successive days might result in a more gradual initial drop. Thus a cross-section at one minute would occur at progressively higher levels. Our selection of a resistance reading after three minutes rather than one minute would obviously accentuate the difference between the two experiments.

That habituation was more marked in Davis's experiment than in ours is borne out by a comparison of the gsr's from day to day. As presented in the writers' previous report, the average relative gsr dropped in four weeks to a value representing 75 per cent of its original magnitude. This is a very slight drop compared with analogous curves presented by

Davis covering only five days, in which the reactions to both signal and onset of noise decreased to about 25 per cent of their initial value. The discrepancy between the two sets of data is especially striking since Davis used an absolute measure of gsr. Had he expressed successive reflexes in terms of the increasing resistances preceding stimulation, the difference would have been accentuated.

We are led to the conclusion that there was less habituation to the electric shock than to the noise. The difference between the stimuli in potential danger suggests a biological explanation in terms of natural selection. It should be recalled, however, that some habituation to shock did occur, which was more marked in the muscular responses. Therefore the gsr may not be regarded as a general index of the organism's habituation to shock.

The question still remains whether in their daily fluctuations the galvanic reactions were related to preceding resistance. This has no bearing on our original supposition; *i.e.*, that a rising resistance level, with the assumption of a positive correlation between resistance and size of gsr, might explain the relatively slight decrease of that measure with repetition. However, it is of interest for an understanding of the galvanic phenomena. To throw light on this problem we obtained the correlations presented in Table IV. Pearson product-moment coefficients were computed throughout. In the *N* column appears the number of days, free of technical defects, entering into each subject's correlations. Owing to the small numbers involved in most cases, probable errors have been omitted. The results are therefore to be considered as merely suggestive of certain possibilities. More weight must be placed on the consistency of relationship within the group than on the amount of relationship for any subject.

The column headed r_{AC} represents the relation between the decline of resistance during the first three minutes and the gsr to the first shock. The first variable was represented by the difference between the initial reading and the reading after three minutes, expressed as a fraction of the initial reading. The second variable was represented by the re-

TABLE IV

CORRELATIONS OF RELATIVE GSR WITH INITIAL DROP AND PRECEDING RESISTANCE

Subject	*N*	Shock I		Shock V
		r_{AC}*	r_{BC}*	r_{BC}*
Zuzu	29	−.35	+.78	−.14
Wek	28	+.15	+.21	−.28
Tif	30	−.43	+.30	−.22
Gib	27	−.45	+.28	+.22
Dar	26	+.09	+.10	+.01
Hel	25	−.58	+.29	−.23
Den	24	−.47	+.47	−.01
Lab	23	−.13	+.19	−.11
Les	52	−.36	+.65	+.55
Pom	42	−.36	+.18	+.04
Yix	53	+.34	−.45	−.45
Bok	55	−.54	+.46	+.44

* $A = \frac{\text{Initial—Preceding}}{\text{Initial}}$.

B = Preceding.

$C = \frac{\text{Preceding—Minimum}}{\text{Preceding}}$.

sistance drop in response to the shock expressed as a fraction of the reading after three minutes; that is, just preceding the shock. It will be noted that nine of the 12 correlations are negative and that they range from −.58 to +.34. They may indicate a slight tendency for a large relative decline of resistance to be followed by a small relative gsr.

The correlations in the column headed r_{BC} were computed between the resistance just preceding the first shock and the reflex to that shock expressed as a fraction of the preceding resistance. Eleven out of the 12 coefficients are positive, and they range from −.45 to +.78. They suggest a slight tendency for higher resistances to be followed by larger relative reflexes. It is possible to interpret this tendency in terms of the factors comprising each variable. If we let B stand for preceding resistance and Min for the minimum resistance reached by the gsr, we are correlating B with B-Min/B. Ruling out the improbable condition that Min might vary inversely with B, a positive correlation could occur only if increases of B were accompanied in general by less than proportional increases of Min. The relative resistance drop is less equivocal in this respect than the absolute drop, since

a positive correlation between B and B-Min might theoretically result from less than proportional, proportional, or even slightly more than proportional increases of Min. In so far as we may infer a positive relationship from our table, we may take it to indicate that as B increases or decreases Min shows a smaller relative increase or decrease.

The first shock, as shown in Fig. 1, was usually given at a point where the resistance curve was still descending perceptibly toward the base-line. This unstable condition of the preceding resistance may be responsible for its slight positive correlation with the gsr. To test this hypothesis, within the limits of our data, we computed the correlations between preceding resistance and relative gsr for the fifth shock. These are presented in the final column of Table IV. Seven of them are negative, five positive; they range from $-.45$ to $+.55$. What grounds we had for assuming a positive relationship from the results of the first shock have disappeared in those of the fifth.

These findings led us to extend our inquiry to recovery. As a measure of recovery we chose the ratio of the maximum resistance reached during one minute after the reflex to the resistance just preceding the stimulus. Thus, in correlating B with Max/B, if Max increases proportionally with B the correlation will be zero; if more than proportionally, positive;

TABLE V

CORRELATIONS OF RECOVERY RATIO WITH PRECEDING RESISTANCE

Subject	N	Shock I r_{B-D}*	Shock V r_{B-D}*
Zuzu	29	−.69	+.50
Wek	28	−.07	−.23
Tif	29	−.10	+.18
Gib	27	.00	−.05
Dar	26	−.40	−.12
Hel	25	−.43	−.55
Den	24	−.43	+.09
Lab	23	−.56	+.06
Les	54	−.24	−.09
Pom	42	+.01	+.27
Yix	51	−.57	−.32
Bok	55	−.32	−.13

* B = Preceding.

$D = \frac{\text{Maximum}}{\text{Preceding}}$.

if less, negative. The correlations for the first and fifth shocks are presented in Table V. For the first shock they range from −.69 to +.01, with 10 out of 12 negative; for the fifth shock they range from −.55 to +.50, with seven negative. On the whole, the table gives the impression of a slight negative relationship for the first shock and of no relationship for the fifth.

These results raise the question of daily variability of resistance preceding the first and fifth shocks. If there is a tendency for a higher resistance preceding the first shock to fall relatively lower in response to it and to recover relatively less, we should expect subsequent resistance, *e.g.*, preceding the fifth shock, to show less variation. That this is true of absolute variability in 11 out of 12 subjects is shown by the σ's in Table VI. The coefficients of relative variation, however, also presented in Table VI, fail to show the expected drop. Apparently any inequalities introduced by the first shock were compensated for in the general lowering of resistance.

TABLE VI

ABSOLUTE AND RELATIVE VARIABILITIES OF PRECEDING RESISTANCES

Subject	Shock I		Shock V	
	σ	V	σ	V
Zuzu	1804	14.67	1664	14.99
Wek	1604	15.08	1192	14.05
Tif	1526	14.37	1459	15.03
Gib	1643	13.59	1367	14.15
Dar	2133	21.18	825	9.31
Hel	754	10.19	558	8.50
Den	1212	11.72	877	9.89
Lab	1202	16.63	1245	19.49
Les	2152	20.57	1889	20.09
Pom	2319	18.67	2209	20.32
Yix	965	11.11	850	10.75
Bok	1565	18.58	1291	18.21

III. DISCUSSION

It is the writers' impression that the keynote in the present findings is the regular, negatively accelerated decline of resistance depicted in Fig. 1. This continued during the anticipatory period and from shock to shock. We believe

that this may be a factor determining the amount of the gsr and of the recovery therefrom.

With direct current and liquid electrodes, a descending curve of resistance has been the type most frequently reported. The factors determining the "curve of rest" may be primarily peripheral, involving physical and physiological changes in the region of the electrodes, or primarily central, involving the psychological reactions of the subject to the situation. Among peripheral conditions, Guilleminot (**6**), Gildemeister (**5**), Wechsler (**9**), and Thouless (**8**) have presented experimental evidence that the passage of external current through the tissues plays a part. On the other hand, Farmer and Chambers (**4**), Thouless (**8**), and Davis (**3**) found that with the subject in contact with the electrodes the fall of resistance was equally rapid whether the current was on or off. This suggests that absorption of liquid by the skin may be a factor. It is possible that both absorption and current flow may involve the penetration of the skin by electrolytic ions [cf. Wechsler (**9**)], leading to a lessened polarization of the outermost cell membranes and a consequent decrease of apparent resistance. The role of emotional excitement in lowering resistance has been emphasized by Thouless (**8**), Cattell (**1**), and Davis (**3**).

The present data are not adapted to analyzing central and peripheral factors. They suggest the assumption, however, that throughout the experimental period the subject's resistance was approaching equilibrium. The equilibrium itself must be physical in nature; the determining factors may be primarily either physical or psychological. It should be noted that, although our data strongly suggest the assumption of a continuous curve, they do not wholly establish it. To be sure that the initial and preceding resistances fall on a single curve we should need more readings during the first three minutes. Granting the assumption, the present data do not prove that the findings are necessarily dependent on this condition. To establish such dependence we should need comparable data from experiments in which the shocks were administered *after* a stable level had been reached.

Admitting the tentative nature of our remarks, we propose to apply the concept of disequilibrium to certain of our results:

1. The decrease of gsr, both absolute and relative, from shock to shock, may be due to habituation. It is also plausible to think of the shocks as being facilitated by some factor or group of factors making for diminished resistance. This effect would be most marked in the early stages when the facilitating factor was most predominant.

2. The slight decrease in absolute recovery from shock to shock is probably related to the decrease in gsr. The increase in recovery relative to gsr may also be attributed to the smaller amount to be recovered, if we assume a tendency for the recovery process to maintain a more or less constant absolute value. Two further explanations are suggested, disregarding psychological speculation: (a) The negative acceleration of the resistance curve. The more rapidly this was falling during the minute following a shock the lower would be the relative maximum permitted. (b) Postulating a factor operating to reduce resistance, such a factor would not only reinforce the shocks but resist recovery. This effect, too, would be strongest in the early stages and diminish as equilibrium was approached.

3. The same condition may be responsible for the day-to-day relationships; *e.g.*, the tendency of the relative gsr to the first shock toward negative correlation with previous resistance drop and positive correlation with preceding resistance. A small initial drop and high resistance after three minutes on a given day may mean that the subject's stabilization process is less complete, thus permitting a larger reflex. The same argument would apply to the negative correlation suggested between the daily resistances preceding the first shock and the relative maximum resistances following. The higher the resistance the farther from equilibrium; the farther from equilibrium the less the relative level of recovery. With the attainment of stability these relationships should disappear. Although stability was not attained within our experimental period, the correlations for the fifth shock were about equally divided between positive and negative. The

smaller absolute variability at the fifth shock may play some part in the differences noted. Although the correlations are too few and unreliable to provide a conclusive test, they lend some support to the proposed interpretation.

4. The data on variability are ambiguous. If daily variations in resistance preceding the first shock represent, in part, variations in progress toward equilibrium, we should expect them to be greater than those at the fifth shock both absolutely and relatively. Actually the σ's decreased, but the V's, on the average, showed no change. The possibility remains that the effects we have tentatively assigned to preliminary instability of resistance may decrease at a rate more than proportional to the decline of resistance itself.

IV. Summary

Analysis of galvanometric data secured in a previously reported investigation yielded the following results:

1. A negatively accelerated decline of resistance during an experimental period of approximately eight minutes.

2. A negatively accelerated decline of the gsr's to five successive electric shocks. This was true both of the absolute gsr's and, as shown in the original report, of the gsr's relative to preceding resistance.

3. A much smaller decrease of absolute recovery from the five shocks. This resulted in an increase of recovery relative to preceding reflex, as shown in the original report.

4. Variation from day to day in the resistance level after three minutes, with no consistent upward or downward trend.

5. A slight tendency toward negative correlation between initial relative resistance drops on different days and relative gsr's to the first shock.

6. A slight tendency toward positive correlation between daily resistances preceding the first shock and relative gsr's in response to it. This tendency fails to appear at the fifth shock.

7. A slight tendency toward negative correlation between daily resistances preceding the first shock and ratios to these of maximum resistances during the following minute. This tendency also fails to appear at the fifth shock.

8. A decrease between the first and fifth shocks in absolute, but not relative, daily variability of preceding resistance.

An interpretation of points 2, 3, 5, 6, and 7 was suggested in terms of the diminishing instability of apparent resistance represented in point 1.

V. Conclusion

For methodology the significance of the present findings is clear. They point to a relation between galvanic reactions and resistance level apparently dependent on the continuous change which that level was undergoing during the experimental period. They may well serve as a reminder that to stimulate the subject before his resistance has reached a steady level introduces a further complication into an already complicated reaction. The factors determining the relationships here presented seem worthy of further experimental investigation.

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THE EFFECT OF KNOWLEDGE OF OUTPUT ON MUSCULAR WORK

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Statement of the Problem

The purpose of this study was to answer three questions: What changes occur in the curve of muscular work when, after having worked in ignorance of the amount of his ouput, the worker is given knowledge of results? What is the effect on the work curve of the discontinuation of knowledge of output? What are the differences, if any, between men and women with respect to the effect of knowledge of output on work?

Previous Investigations

Knowledge of results as a motivating factor has received considerable experimental study. The effect of knowledge of results on estimating the size of angles has been studied by Judd (**12**) and by Spencer (**15**), on the making of tally marks by Ross (**13**), on achievement in elementary school subjects by Chapman and Feder (**4**) and by Brown (**3**) and on grades made by college freshmen in philosophy by Deputy (**6**). These investigations agree in finding that knowledge of results is followed by an increase in the functions studied. In contrast with his earlier laboratory experiment, however, Ross (**14**) found that in the college classroom knowledge of progress had a negligible effect upon achievement. In a study of the influence of incentive and punishment upon reaction time, Johanson (**11**) concluded that telling the subject the length of his reaction time was "a powerful factor in reducing a normal reaction time, which ordinarily is supposed to be as fast as the subject is able to react." Knowledge of results was included among the incentives studied by Hamilton (**8**) in his investigation of the effect of incentives on accuracy of discrimination measured on the Galton bar. Hamilton found that

"visual discrimination of horizontal lengths by the method of average error was made more accurate by knowledge of results."

The first systematic study of the effect of knowledge of output on muscular work is one carried out by Arps. In a preliminary report, Arps (1) stated that the average absolute efficiency value of all the work series done with knowledge of results exceeded that of the series done without knowledge of results by ten percent. In his later paper, Arps (2) concluded that "the average unit value of the known series of all sets of experiments shows a higher rate of work by 18 percent over the corresponding value of the unknown series." Crawley (5) found that all of his four subjects produced greater amounts of muscular work when they were permitted to watch their own performance than they did when required to work without knowledge of their output.

Subjects and Apparatus.—The subjects in this experiment were sixty-eight men and sixty women college students between the ages of eighteen and twenty-five years. The subjects of each sex were divided into an experimental and a control group. The work studied consisted of maximal contractions on the Smedley hand dynamometer. In using this instrument, the subject squeezes the two handles with his maximum force. The work done is registered in kilograms by a pointer which moves in an arc over a graduated dial.

Procedure.—Each subject in the control groups made fifty maximal contractions at intervals of fifteen seconds, all without knowledge of how much work was done. With the experimental groups, the procedure was as follows. The whole work series was divided into three parts. Contractions one through ten were made *without* knowledge of results. Contractions eleven through thirty: during the fifteen-second rest between the tenth and the eleventh contraction, the experimenter informed the subject that he would be told how much work he did after each of the succeeding contractions. Contractions thirty-one through fifty: beginning with the thirty-first contraction, the experimenter ceased telling the subject his work score. The last twenty contractions comprise work done after knowledge of results had been discontinued.

The work was done by each subject individually in a small room, free from noise and other distractions. While doing the work, the subject was seated at a table. The dynamometer was on the table, clamped vertically to an iron stand which was free to move over the top of the table at the convenience of the subject. Both the apparatus and the record were completely hidden from all subjects in both experimental and control groups throughout the experiment. After each contraction was made, the reading on the dial of the dynamometer was recorded by the experimenter and the pointer was set back to zero. Each subject was given a practice period long enough to familiarize him with the apparatus and the procedure, but not so long as to produce fatigue.

Results

Since each subject made fifty contractions, the data of this study consist of fifty frequency distributions. Among the men subjects, each distribution consists of thirty-four work scores; among the women subjects, each distribution has thirty work scores. For each of the fifty distributions in the work series, the following measures have been found: mean, standard deviation coefficient of variation, difference between the mean of the experimental group and that of the control group, the critical ratio and the chances in one hundred that the true difference is greater than zero. The computations in Table I are based on the work done in the first ten contractions

TABLE I

Work Done in Contractions One Through Ten—Without Knowledge of Results

Men Subjects

Number of Contraction	Experimental Group N = 34			Control Group N = 34			Difference between Means	Critical Ratio	Chances in 100
	Mean (Kilos)	S. D.	C. V.	Mean (Kilos)	S. D.	C. V.			
1	47.40	7.42	15.65	48.26	8.81	16.95	0.86	0.67	67
2	46.88	8.19	17.63	46.88	9.49	20.24	0.43	0.30	58
3	45.48	7.62	16.75	45.74	7.87	17.21	0.26	0.21	55
4	44.32	6.78	15.30	45.29	7.91	17.47	0.97	0.80	71
5	44.41	8.06	18.15	44.35	9.05	20.41	0.06	0.04	51
6	42.96	8.85	20.60	43.82	8.18	18.67	0.86	0.62	66
7	42.29	8.95	21.16	43.74	8.18	18.70	1.45	1.03	76
8	42.22	8.63	20.44	42.76	7.62	17.82	0.54	0.41	61
9	42.26	8.77	20.75	42.03	8.89	21.15	0.23	0.16	54
10	41.57	6.48	15.59	41.55	5.83	14.03	0.02	0.02	50

Women Subjects

Number of Contraction	Experimental Group N = 30			Control Group N = 30			Difference between Means	Critical Ratio	Chances in 100
	Mean (Kilos)	S. D.	C. V.	Mean (Kilos)	S. D.	C. V.			
1	29.70	6.88	23.16	29.47	5.57	18.90	0.23	0.21	55
2	28.53	6.50	22.78	29.33	5.29	18.04	0.80	0.78	71
3	27.97	6.30	22.52	27.13	5.12	18.87	0.84	0.84	72
4	26.30	5.65	21.48	26.90	5.67	21.08	0.60	0.61	66
5	25.63	5.72	22.32	27.10	4.82	17.79	1.47	1.61	86
6	25.77	5.27	20.45	26.50	4.89	18.45	0.73	0.82	71
7	25.83	5.63	21.80	26.17	4.85	18.53	0.34	0.37	59
8	25.53	5.01	19.62	25.40	4.76	18.74	0.13	0.15	54
9	25.47	5.47	21.48	25.13	5.01	19.94	0.34	0.37	59
10	25.17	4.79	19.03	25.30	4.63	18.30	0.13	0.16	54

—without knowledge of results; the figures in Table II are based on the work done in contractions eleven through thirty, during which each subject in the experimental groups was told after each contraction how much work he did and Table III contains the calculations based on contractions thirty-one through fifty, during which knowledge of results had been discontinued.

Equating of the Groups.—The means and the critical ratios in Table I show that throughout the first ten contractions, during which knowledge of results was withheld from all subjects, the curves of work of the experimental and of the control groups are close together. The mean critical ratio of the difference between the mean of the experimental group and that of the control in the first ten contractions is, for the men subjects, .43, and for the women subjects, .59. These critical ratios indicate that the experimental and the control groups had essentially the same initial muscular strength.

The Introduction of Knowledge of Output.—Knowledge of results, which began among the experimental groups at the eleventh contraction, is followed by a prompt change in the trend of the work curve. In Table II the means of the experimental groups rise rapidly and then gradually decline. Among the women subjects, the effect of knowledge of output is greater and more sudden than among the men. When the means of the experimental groups are compared with those of the control groups, the largest critical ratio among the women subjects, 4.15, is found at the fifteenth contraction. The largest critical ratio among the men is 3.37, which comes at the twenty-first contraction.

In the procedure it will be recalled that, after the tenth contraction had been made, the subjects in the experimental groups were informed by the experimenter that, beginning with the eleventh contraction, they would be told how much work was done in each subsequent contraction. Among the women subjects, the critical ratio at the tenth contraction is 0.16, while at the eleventh contraction the critical ratio rises to 2.82. The corresponding critical ratios among the men subjects are 0.02 and 0.64. This sudden increase in output

TABLE II

WORK DONE IN CONTRACTIONS ELEVEN THROUGH THIRTY—WITH KNOWLEDGE OF RESULTS

Men Subjects

Number of Contraction	Experimental Group N = 34			Control Group N = 34			Difference between Means	Critical Ratio	Chances in 100
	Mean (Kilos)	S. D.	C. V.	Mean (Kilos)	S. D.	C. V.			
11	42.74	8.31	19.44	41.88	8.06	19.25	0.86	0.64	67
12	43.57	7.48	17.17	41.29	8.49	20.56	2.28	1.74	88
13	43.55	7.55	17.34	40.62	6.63	16.32	2.93	2.52	95
14	44.21	10.68	24.16	41.29	9.75	23.61	2.92	1.75	88
15	43.84	9.80	22.35	40.88	8.55	20.91	2.96	1.97	91
16	44.16	10.82	24.50	40.35	9.38	23.25	3.81	2.30	94
17	44.11	10.20	23.12	40.38	9.85	24.39	3.73	2.27	94
18	43.62	7.68	17.61	40.65	6.00	14.76	2.97	2.63	96
19	43.51	7.21	16.57	39.38	8.54	21.69	4.13	3.19	98
20	42.59	6.86	16.11	39.23	7.83	19.96	3.36	2.79	97
21	42.58	7.48	17.57	38.65	6.74	17.44	3.93	3.37	99
22	42.34	6.48	15.30	39.79	7.00	17.59	2.55	2.31	94
23	41.85	8.66	20.69	39.44	9.20	23.33	2.41	1.65	87
24	41.85	8.18	19.55	39.32	9.75	24.80	2.53	1.72	87
25	41.50	8.71	20.99	39.35	9.54	24.24	2.15	1.44	84
26	41.86	8.78	20.97	39.15	9.22	23.55	2.71	1.84	89
27	42.24	9.64	22.82	38.82	8.00	20.61	3.42	2.36	95
28	42.43	6.16	14.52	38.79	7.48	19.28	3.64	3.25	99
29	41.71	8.00	19.18	38.85	8.06	20.75	2.86	2.18	93
30	41.00	8.89	21.68	38.00	9.00	23.68	3.00	2.05	92

Women Subjects

Number of Contraction	Experimental Group N = 30			Control Group N = 30			Difference between Means	Critical Ratio	Chances in 100
	Mean (Kilos)	S. D.	C. V.	Mean (Kilos)	S. D.	C. V.			
11	26.87	4.51	16.78	24.47	5.25	21.45	2.40	2.82	97
12	27.20	4.85	17.83	24.87	4.57	18.38	2.33	2.36	95
13	27.27	4.40	16.13	24.25	4.72	19.46	3.02	3.80	99
14	26.87	4.99	18.57	23.97	4.97	20.73	2.90	3.34	99
15	27.33	4.47	16.36	24.07	4.56	18.94	3.26	4.15	100
16	27.37	5.06	18.49	23.83	5.03	21.11	3.54	4.02	100
17	26.43	5.93	22.44	23.63	4.86	20.57	2.80	2.97	98
18	26.50	5.35	20.19	23.70	4.86	20.51	2.80	3.15	98
19	26.37	5.28	20.02	23.53	4.53	19.25	2.84	3.31	99
20	26.23	5.01	19.10	23.60	4.19	17.75	2.63	3.27	99
21	26.33	4.51	17.13	23.77	4.13	17.37	2.56	3.40	99
22	25.60	4.71	18.40	23.13	4.37	18.89	2.47	3.12	98
23	25.67	4.53	17.65	22.90	4.49	19.61	2.77	3.53	99
24	25.63	5.07	19.78	22.70	4.72	20.79	2.93	3.43	99
25	25.90	5.26	20.31	22.63	5.02	22.18	3.27	3.65	99
26	24.93	5.75	23.06	22.77	5.76	25.30	2.16	2.16	93
27	25.87	5.44	21.03	23.10	5.08	21.99	2.77	3.02	98
28	25.50	5.10	20.00	23.60	5.68	24.07	1.90	2.02	91
29	25.53	5.05	19.78	23.13	5.14	22.22	2.40	2.70	97
30	25.57	4.55	17.79	22.63	5.38	23.77	2.94	3.39	99

among the experimental group of the women subjects cannot be regarded as evidence of the effect of knowledge of output, since no knowledge of results had been given to these subjects. This rise in output is evidence, rather, of the effect of the expectation that such knowledge was about to be received. The same extent of increase among the men subjects is not reached until the nineteenth contraction. Since the expectation of knowledge of results alters the trend of the work curve, it may be inferred that the effectiveness of an incentive, such as knowledge of output, depends in part at least upon the subject's understanding of its significance. In connection with a similar finding in his study of incentives, Hamilton (9) expresses the belief that "the physical stimuli used in creating the incentives do not in themselves constitute the incentives . . . The incentive itself may be an interpretation placed upon the stimulus by the subject."

The Discontinuation of Knowledge of Output.—As soon as knowledge of output is discontinued (at the thirty-first contraction), the work curves of the experimental groups fall more rapidly than they did while the subjects were being informed of their output. In Table III it will be observed, however, that, throughout the rest of the work series, the means of the experimental groups do not decline to the level of the means of the control groups. The preceding incentive appears to have an after-effect which supports the experimental work curve at a level above that of the control curve throughout the rest of the work series. This after-effect is somewhat more marked among the men subjects than among the women. As the output of the women subjects was more promptly affected by the onset of incentive than that of the men, so the output of the women subjects falls more rapidly after the discontinuation of knowledge of results than that of the men.

The standard deviations and coefficients of variation in Tables I, II, and III indicate that knowledge of output has no systematic effect on either absolute or relative variability. No difference has been found between the men and the women subjects with respect to the effect of this incentive on variability.

TABLE III

WORK DONE IN CONTRACTIONS THIRTY-ONE THROUGH FIFTY—AFTER DISCONTINUATION OF KNOWLEDGE OF RESULTS

Men Subjects

Number of Contraction	Experimental Group N = 34			Control Group N = 34			Difference between Means	Critical Ratio	Chances in 100
	Mean (Kilos)	S. D.	C. V.	Mean (Kilos)	S. D.	C. V.			
31	41.09	9.64	23.46	38.15	9.00	23.59	2.94	1.93	91
32	39.94	9.00	22.53	38.26	9.33	24.39	1.68	1.12	77
33	39.94	9.11	22.81	37.47	10.39	27.73	2.47	1.55	85
34	40.11	9.43	23.51	38.15	8.60	22.54	1.96	1.33	82
35	39.97	9.17	22.94	37.38	10.15	27.15	2.59	1.64	87
36	39.66	7.21	18.18	37.62	6.16	16.37	2.04	1.86	89
37	39.35	6.21	15.78	37.47	7.68	20.50	1.88	1.65	87
38	39.82	7.35	18.46	35.68	7.21	20.21	4.14	3.48	99
39	39.40	7.69	19.51	37.26	8.17	21.93	2.16	1.66	87
40	39.20	8.77	22.37	36.50	7.48	20.49	2.70	2.02	91
41	38.55	7.40	19.20	36.47	8.78	24.07	2.08	1.57	85
42	38.94	8.49	21.80	37.76	7.94	21.03	1.18	0.88	73
43	38.98	8.49	21.78	36.68	7.48	20.39	2.30	1.76	88
44	38.95	8.54	21.93	36.65	7.75	21.15	2.30	1.72	87
45	39.53	6.40	16.19	37.15	8.54	22.99	2.38	1.93	91
46	39.47	6.00	15.20	36.24	8.85	24.42	3.23	2.61	96
47	39.10	8.38	21.43	36.53	7.40	20.26	2.57	1.99	91
48	38.12	8.89	23.32	36.21	8.33	23.00	1.91	1.36	82
49	38.70	7.35	18.99	35.88	8.43	23.49	2.82	2.18	93
50	39.16	8.43	21.53	35.94	8.54	23.76	3.22	2.32	94

Women Subjects

Number of Contraction	Experimental Group N = 30			Control Group N = 30			Difference between Means	Critical Ratio	Chances in 100
	Mean (Kilos)	S. D.	C. V.	Mean (Kilos)	S. D.	C. V.			
31	25.23	4.29	17.00	22.87	4.71	20.59	2.36	3.01	98
32	24.87	4.54	18.25	23.30	5.04	21.63	1.57	1.88	90
33	24.50	4.66	19.02	22.47	5.03	22.39	2.03	2.40	95
34	24.20	5.06	20.91	22.23	4.67	21.01	1.97	2.32	94
35	23.53	5.01	21.29	22.73	4.62	20.33	0.80	0.95	74
36	24.07	4.89	20.32	21.60	5.06	23.43	2.47	2.85	97
37	23.80	4.51	18.95	22.50	5.02	22.31	1.30	1.56	85
38	23.03	4.37	18.97	22.80	4.62	20.26	0.23	0.29	58
39	24.40	4.39	17.99	22.43	5.04	22.47	1.97	2.39	95
40	23.87	4.64	19.44	22.40	5.19	23.17	1.47	1.71	87
41	23.47	4.18	17.81	22.60	5.36	23.72	0.87	1.04	76
42	23.30	4.13	17.73	22.27	4.52	20.30	1.03	1.37	82
43	22.67	4.40	19.41	21.90	4.80	21.92	0.77	0.96	74
44	23.20	4.75	20.47	21.50	5.12	23.81	1.70	1.98	91
45	22.97	4.38	19.07	21.20	4.64	21.89	1.77	2.25	94
46	23.43	4.03	17.20	21.50	4.77	22.19	1.93	2.51	95
47	22.83	4.95	21.68	22.20	5.34	24.05	0.63	0.70	68
48	23.33	5.10	21.86	21.57	5.26	24.39	1.76	1.95	91
49	23.27	4.52	19.42	21.37	4.85	22.70	1.90	2.33	94
50	22.97	4.64	20.20	21.93	4.52	20.61	1.04	1.30	81

Relationship between Muscular Strength and Increase in Output Due to Knowledge of Results.—What is the relationship, if any, between the muscular strength of the subjects and their susceptibility to the incentive effect of knowledge of output? The amount of work done in the first contraction has been taken as the measure of muscular strength. The measure of incentive effect is the difference between the amount of work done in the tenth contraction and that done in the contraction in which the effect of knowledge of output has been found to be the greatest. Among the men subjects, the maximal effect was found at the twenty-first contraction; among the women subjects, at the fifteenth contraction. The correlation(rank difference method) between strength and susceptibility to incentive has been found for the four groups of subjects. Among the control group of men subjects, the correlation between these two measures is $-.15 \pm .11$; among the experimental group of men subjects, the correlation is $-.27 \pm .11$. Similarly, among the control group of women subjects, the correlation is $-.17 \pm .12$; among the experimental group of women subjects, it is $-.28 \pm 12$. None of these coefficients is statistically significant. It may be concluded, therefore, that among these subjects there is no significant relationship between muscular strength and increase in output due to the incentive effect of knowledge of output.

The Cumulative Effect of Successive Incentives.—Knowledge of results is, obviously, only one of several incentives whose effect on the curve of work can be studied.[1] How would the work curve be affected, if another incentive were added to that already developed by knowledge of output? In this connection, the present study affords evidence incidental to the main problem.

After the fiftieth contraction had been made, each subject in *both* experimental and control groups was requested to "make just one more contraction, his very best, this one to be the last he would be asked to make." How the mean of this final 'best' contraction compares with the mean of the first

[1] Diserens and Vaughn (7) and Hurlock (10) have summarized the literature of motivation and incentive.

contraction can be seen in Table IV. The mean of the final contraction more nearly approaches that of the first contraction among the experimental groups than among the control groups. The difference between the mean output of the final contraction and that of the first contraction of the experi-

TABLE IV

COMPARISON BETWEEN THE MEAN OUTPUT IN THE FIRST CONTRACTION AND THAT IN THE FINAL CONTRACTION
(KILOGRAMS)

	Men Subjects		Women Subjects	
	Experimental Group N = 34	Control Group N = 34	Experimental Group N = 30	Control Group N = 30
First Contraction	47.40	48.26	29.70	29.47
Final Contraction	45.12	42.52	28.70	25.97
Difference	2.28	5.74	1.00	3.50
Critical Ratio	1.77	4.59	1.00	4.04
Chances in 100	88	100	75	100

mental has a critical ratio of 1.77 for the men and of 1.00 for the women. These critical ratios show that the incentive of being asked to do one's 'best and last' raises the mean of the final contraction of the experimental groups to a level that is almost as high as the mean of the first contraction. Among the control subjects, the corresponding critical ratios are, for the men, 4.59, and for the women, 4.04. These critical ratios indicate that, even though the subjects in the control groups were also asked to do their 'best and last,' nevertheless, the mean output of the final contraction remained significantly below the mean of the first contraction. This finding suggests the possibility that the incentive of making one more 'best and last' contraction, when it is added to the incentive already developed in the subjects by knowledge of output, produces a greater increase in mean output than that which follows the incentive of doing one's 'best and last' in the absence of a preceding incentive.

SUMMARY AND CONCLUSIONS

Sixty-eight men and sixty women college students, divided equally into experimental and control groups, made fifty

maximal muscular contractions each at fifteen-second intervals on the Smedley hand dynamometer. The work done was concealed from all subjects. After each contraction from the eleventh to the thirtieth inclusive, the subjects in the expermental groups were told by the experimenter the amount of work that the subject performed.

The following conclusions seem to be justified:

1. Knowledge of output is followed by a prompt upward turn in the curve of muscular work. The means of the experimental groups rise rapidly and then gradually decline.

2. The effect of knowledge of output is more sudden among the women subjects than among the men subjects.

3. When knowledge of output is suddenly discontinued, there is no abrupt fall in the work curve. The incentive developed by knowledge of results appears to have an after-effect, which holds the means of the experimental groups above those of the control groups throughout the rest of the work series.

4. This after-effect is more persistent among the men than among the women subjects.

5. Telling the subject that he is going to receive knowledge of his output, before the knowledge of his work score begins, results in a prompt upward turn in the curve of work. This anticipatory effect is more pronounced among the women subjects than among the men.

6. No significant relationship has been found among the present subjects between muscular strength and susceptibility to the incentive effect of knowledge of output.

7. Incidental evidence suggests that the effect of different incentives on muscular work may be cumulative.

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THE DIRECT ACTION OF REWARDS UPON MENTAL CONNECTIONS AND THEIR INDIRECT ACTION VIA THE STIMULATION OF INNER EQUIVALENTS OF THE CONNECTIONS

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In human subjects who are learning connections which can be repeated as such or in the form of some inner equivalent, the confirming reaction causes a tendency to repeat a connection which it confirms. If the situation persists, the subject tends to repeat the rewarded response to it. If the situation vanishes, the subject may call to mind some inner equivalent of it and connect the rewarded response with that. Various means have been used to exclude such inner repetition in cases where we need to know the direct and immediate action of the reward upon the connection. It is also desirable to know how much allowance needs to be made for such inner repetition in cases where it has not been excluded; and I have made an experiment providing such knowledge in a typical case.

In this experiment, the time per unit was regulated by a click occurring every 2½ seconds, the task was to respond to a word by some number from 1 to 10, and the after-effect was 'Right,' 'Close,' or 'Wrong.' No money rewards were given. Twenty series, each containing 40 word—number pairs, were used, with eighteen subjects, all educated adults. Each series was used three times. In the first five series the subject was left to his own devices. In the next ten he was instructed to refrain from any inner repetition of any numbers during the learning. In the last five he was instructed as follows:

"For the remainder of the experiment, please do as follows in the trials in which I announce 'Right,' 'Close,' or 'Wrong.' Whenever I call the number you said 'Right,' think to yourself once '4 (or whatever the number was) is right for bread (or whatever the word was)' if you have time to do so before I say

the next word. Whenever I announce 'Close,' say to yourself once '5 (or whatever the number was) is close for bread (or whatever the word was).' Whenever I announce 'Wrong,' think to yourself once '6 (or whatever the number was) is wrong for bread (or whatever the word was).'

"That is, instead of trying *not* to say to yourself that the number is right or close or wrong, you will now try to say to yourself whenever you can

..... is right for ....., or
..... is close for ....., or
..... is wrong for ....."

After the fifth series he was questioned concerning the frequency of such inner repetitions, as follows:

"1. In the experiment so far, when I called your number 'Right' did you ever repeat the number to yourself?..... If so, how often? Almost always..... About 3/4 of the time..... About half the time..... About a quarter of the time..... Very very rarely.....

"2. When you repeated a right number to yourself, did you ever repeat also the word it belonged with?..... If so, how often? That is, in what percent of the times that you said the number to yourself did you say the word also. 100%..... 90%..... 80%..... 70%..... 60%..... 50%..... 40%..... 30%..... 20%..... 10%..... less than 10%.....

"3. When I said 'Wrong,' did you ever repeat to yourself '6 is wrong' (or '10 is wrong' or '4 is wrong,' or whatever the number was)?..... If so, how often? Almost always..... About 3/4 of the time..... About half of the time..... About 1/4 of the time..... Very, very rarely.....

"4. When you said to yourself 'So and so is wrong,' did you ever say to yourself or think also what word it was wrong for?..... If so, in what percent of the cases when you repeated '3 is wrong' or '4 is wrong,' etc. did you also repeat the word it was wrong for? 100%..... 90%..... 80%..... 70%..... 60%..... 50%..... 40%..... 30%..... 20%..... 10%..... less than 10%.....

"5. Did you say or think anything on the occasions when I said 'Close'?..... If so, what was it?....."

Similar questions were asked after the fifteenth series and after the twentieth.

We have individuals who, when left to their own devices, used such inner repetition not at all or in varying amounts, and can observe what happened to their learning when the amount was reduced and when it was increased. The most convenient measures of learning are the gain in 'Rights' from Trial 1 to Trial 3, and the gain in 'Rights plus Closes' from Trial 1 to Trial 3.[1]

Consider first the facts for series 1 to 5 in which the subjects were left to their own devices. There were 8 subjects who reported that they never repeated numbers to themselves in these five series, and 1 who reported that he did so once. Their average gain in Rights was 2.05; in Closes 0.49.

There were 4 who reported that they repeated numbers called right in 25 percent of the cases, and never repeated numbers called wrong. One of them sometimes would think "9 is right" if 10 was called close, and "2 is right" if 1 were called close. Their average gain was 2.15 in Rights, and −0.20 in Closes.

There were 5 who reported that they repeated numbers called right in 50, 50, 75, 75, and 90 percent of the cases. One of them repeated numbers called wrong in 25 percent of the cases. They often repeated a number that was called close, trying to remember to go up or down 1 step the next time. If 1 (or 10) was called close, one of them would say to himself 2 (or 9) is right. Their average gain in rights was 2.36; in closes 0.20.

So far as these data go, they indicate a small and unreliable advantage from inner repetition.

Consider next the results when the subjects were instructed "Do not ever repeat anything to yourself or think in any way concerning any number that you say after you have said it. Say the number, listen to my announcements of 'Right' or

[1] The trials referred to here as 1, 2, and 3 were preceded by two trials with no after-effects given by the experimenter. This was done for another purpose and is of no importance for the problem under discussion.

'Close' or 'Wrong' when I make them, and then think of nothing until you hear the next word." In the ten series under this régime, the 9 subjects who had not indulged in inner repetition when left to their own devices did as before, except that the suggestion that numbers could be repeated caused a few involuntary repetitions in some who had had none before. Their average gains were 2.39 in rights and –0.01 in closes. They thus learned about as well as before.

The 4 subjects who reported 25 percent repetition of numbers called right in series 1 to 5, reduced this to 0, 0, 10, and 25 percent in series 6 to 15. Their average gain was 1.05 in rights and 0.28 in closes. They apparently suffered from the restriction. The 5 subjects who reported 50 to 90 percent repetition in series 1 to 5 reduced this to 0, 0, 0, 1, and 20 percent in series 6 to 15. Their average gain was 1.60 in rights and –0.12 in closes. They also apparently suffered from the restriction. Combining the results, the nine who originally repeated numbers dropped from an average gain of 2.27 in rights and 0.01 in closes to 1.36 in rights and 0.05 in closes.

It is, however, unlikely that this difference measures the amount of learning to be credited to inner repetition. Much, possibly most, of it may be due to the general ill effect of having to devote attention to following the instructions. Indeed, the two subjects who did not eliminate repetitions showed a greater drop than the seven who did.

Consider now the results when the subjects were urged to use inner repetition of "..... is right for ....., ... is wrong for ....., ... is close for ....." as much as they could. Of the 9 who had not done so previously, 4 now did after about 70 percent of the rights and about 35 percent of the wrongs. They did only a little better than they had done in series 1 to 5. Four still did not repeat. They also did a little better than in series 1 to 5. Of the 9 who had repeated in series 1 to 5, all now repeated, on the average more than in trials 1 to 5. They learned a little more than in trials 1 to 5. The essential facts are given in detail in Table I.

We may obtain a convenient picture of the entire event by comparing the two groups of nine using (I) the average gains

TABLE I

LEARNING WITH NO SPECIAL INSTRUCTIONS (NORMAL), WITH INSTRUCTIONS NEVER TO REPEAT THE CONNECTIONS (NO. REP.), AND WITH INSTRUCTIONS TO REPEAT THE CONNECTIONS AS MUCH AS POSSIBLE (REP. MAX.)

	Normal: Series 1–5		No. Rep.: Series 6–15		Rep. Max.: Series 16–20	
	Gain in R	Gain in C	Gain in R	Gain in C	Gain in R	Gain in C
Nine persons who normally did not repeat:						
Mb	8	1	19	− 2	12	1
We	20	− 3	54	−13	17	4
Bl	6	− 3	24	16	23	−13
Ta	11	5	25	−10	38	−23
Me	12	− 1	33	1	27	6
Jo	4	12	22	− 7	16	− 4
La	5	10	−8	16	2	7
Da	7	7	10	− 1	15	−10
Bu	19	− 6	36	− 1	3	− 1
Four persons who normally repeated right responses 25%:						
Da	13	1	1	16	8	− 5
We	7	7	9	0	5	0
Cr	15	− 4	11	4	18	8
Pu	8	− 8	21	9	17	3
Five persons who normally repeated right responses 50, 50, 75, 75, and 90%:						
Co	8	13	4	−17	9	0
Kr	23	−14	25	−15	5	14
Li	−6	9	4	33	10	− 4
Pa	16	8	16	0	3	14
Pe	18	−11	31	− 7	6	4

in rights plus closes, or (II) the average gains in rights plus half the closes. The facts are:

	Normal	Instructed Not to Repeat	Instructed to Repeat
For 9 not normally using inner repetition— I	2.55	2.40	2.67
II	2.29	2.40	3.33
For 9 that normally used inner repetition— I	2.28	1.41	2.56
II	2.28	1.38	2.18

The two groups learn equally well when left to their own devices. The first group lose nothing by the restriction and gain perhaps 15 percent by being urged to repeat. The second

group suffer severely from the restriction and gain perhaps 5 percent by being urged to repeat. The amount of strengthening of the connections that can reasonably be attributed to the inner repetition is then perhaps 10 percent. This percentage is, of course, not highly reliable, but there is no greater likelihood of its rising than of its falling.

The strengthening by the attachment of the satisfying after-effect in multiple-choice experiments in which a new situation follows very soon after the after-effect of the previous connection is almost exclusively due to direct action on the connection, not to the incitement of an inner repetition of it. A substantial percentage of subjects do not thus repeat, and those who do repeat gain little from doing so.

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THE EFFECT ON TACTUAL LOCALIZATION OF MOVEMENT DURING STIMULATION

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I. The Problem

The purpose of this investigation was to determine what effect there would be on the constant error of touch localization if the tactual stimulus was presented while the bodily member stimulated was in motion. Certain authors have suggested that the typical errors of localization which are characteristic of given areas are due to the sort of movement which usually occurs when these areas are stimulated. The most outstanding descriptions of these movements have been contributed by W. James and E. B. Holt.

W. James [1] asserts that there is a tendency to move the extremities or other mobile parts, when they are touched, so that the most sensitive part of the member will be brought into contact and be allowed to explore the touching object. The most sensitive part of a member is usually its tip, *e.g.*, the finger-tips, toes, or tip of tongue. And so James says,

"There thus arise lines of habitual passage from all points of a member to its sensitive tip. These are the lines most readily recalled when any point is touched, and their recall is identical with the consciousness of the distance of the touched point from the 'tip.'"

E. B. Holt [2] offers the view that judgments of tactual space are based on the movements which are most frequent and most natural anatomically for any given segment.

This view depends upon certain considerations regarding the genetic development of behavior in the individual. To begin with, Holt believes that many of the first definite responses appearing out of the diffuse, random movements of infancy are 'reflex circles.' These are responses which become conditioned to sensory impulses aroused by the response itself and which tend to persist or be repeated, such as the infant's reiteration of vocal sounds. The important aspect in the establishment of these

[1] W. James, *Principles of Psychology*, 1890, II, 160–161.

[2] E. B. Holt, *Animal Drive and the Learning Process*, 1931, I, Chapters VII and X.

responses, according to Holt, is that they "go out to meet the stimulus, get more of it, repeat or reproduce the stimulation."[3] They are 'adient.' The principle of adience is applied to space perception. In any limb movement the foremost surface will receive the hardest impact and surfaces to the side milder contact. Holt says that "in time a pressure applied to any point of this front profile strip will cause the limb (as a whole) to move toward the pressure (adiently): while a pressure applied on either of the side surfaces (lying more nearly tangential to the considered plane of movement) will innervate the limb (but less intensely than in the previous case) to move in the same original plane: though this is now more or less sidewise to the direction of pressure on the surface now stimulated."[4] All points of the limb will eventually act as front and as lateral surfaces; pressure at any point will eventually arouse 'a considerable motor pattern.' The points which figure most often as front surfaces are determined by the combined action of the joints of the limb, *e.g.*, to the action of the shoulder may be added the extension and flexion of the forearm with the result that the volar and palmar surfaces receive the most frequent impacts. The more distal that a point is on a limb the greater will be the momentum and the stronger the sensory excitation occurring while the movement is still in progress. Also, "since the movement of the limb along the line of its own axis is anatomically difficult and restricted, the innervation components acquired for movements in this direction will be relatively meagre. Such movements do occur at random, and such innervations are bound to develop, as for instance on the forearm surfaces when the elbow is partially flexed and the shoulder joint inactive. Yet the 'education' of the skin for longitudinal movements of the limbs will lag behind that for transverse movements."[5] Thus Holt concludes, "This motor education on limb surfaces presents at once a parallelism with our tactual consciousness of space. It has long been known that the accuracy with which a touch stimulation can be localized by the person touched and the ability to discriminate two near-lying simultaneous pressures *as two* (two-point limen, Weber's circles) *vary directly with the motility* of the surface touched (Vierordt, 1870; Paulus, 1871; Riecker, 1873; V. Henri, 1898, pp. 123–5)."[6] "On the upper- and fore-arm, thigh and lower leg there is but little reflex movement in the direction of the limb's axis, and in this particular direction the discrimination in less keen (Ebbinghaus, 1902, p. 443; Ponzo, 1911, p. 13; 1912): so that Weber's 'circles' are on most parts of the limbs ellipses of which the long axes lie parallel to the axis of the limb."[7]

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[3] *Ibid.*, 41.

[4] *Ibid.*, 74.

[5] *Ibid.*, 75.

[6] *Ibid.*, 78. Cf. K. Vierordt, Die Abhängigkeit der Ausbildung des Raumsinnes der Haut von der Beweglichkeit der Körperteile, *Zsch. f. Biol.*, 1870, **6**, 53–72. A. Paulus, Versuche über den Raumsinn der unteren Extremitäten, *Zsch. f. Biol.*, 1871, **7**, 237–262. A. Riecker, Versuche über den Raumsinn der Haut des Unterschenkels, *Zsch. f. Biol.*, 1873, **9**, 95–103. V. Henri, *Über die Raumwahrnehmungen des Tastsinnes*, 1893, 123–5; (Henri's demonstration on these pages of the influence of *Anhaltspunkte* differs somewhat from Vierordt's observations).

[7] *Loc. cit.* Cf., H. Ebbinghaus, *Grundzüge der Psychologie*, 1902, I, 443; M. Ponzo, Recherches sur la localisation des sensations tactiles et des sensations dolorifiques, *Arch. Ital. de Biol.*, 1912, **58**, 231–244; (Ponzo's method of determining the constant error has been seriously criticized by S. I. Franz, The constant error of touch localization, This Journal, 1916, **1**, 83–98).

In contrast to the suggestion given by James that the characteristic localizations upon the limbs are due to the habitual reference to points of greatest sensitivity, especially in the distal direction, the view given by Holt suggests that the gross longitudinal range of localization is due to a lack of habit! These two views, so far as the present writer knows, represent the most complete descriptions of the localizing response in the stimulated member. Other suggestions regarding the response of the stimulated member have been given with less detail and specificity.

E. H. Weber states several times that tactual space discrimination depends upon cutaneous end-organs and upon bodily movement (". . . non solu tactu, sed simul motu . . ."),[8] yet he maintains a distinction between movement and what he calls '*Ortsinn*,' saying that we learn about the motion of our limbs from the *Ortsinn* of the skin and at the same time we learn to orient ourselves on our skin through voluntary movements; both capacities come to full development through each other.[9] Nevertheless it is certain that Weber favored an anatomical basis for his 'sensory circles.' In fact K. Vierordt[10] first announced his discovery of the relation between localization and mobility as a piece of evidence directly contradictory to Weber's idea that localization depends on the distribution of nerve plexi. At this time Vierordt states that he had not gone into the basic reasons for the relation. R. H. Lotze[11] bases his theory of tactual local signs upon a de-

[8] E. H. Weber, *De Tactu*, 1834, 145.

[9] *Idem*, Der Tastsinn und das Gemeingefühl, *Wagner's Handwörterbuch der Physiologie*, 1846, 493.

[10] K. Vierordt, Ueber die Ursache der verschiedenen Entwickelung des Ortsinnes der Haut, *Pflüger's Arch. f. d. ges. Physiol.*, 1869, **2**, 297–306. V. Henri (*op. cit.*, p. 27) gives the caution that "Dieses Gesetz von Vierordt kann allerdings nur als angenäherte betrachtet werden." An interesting example of the application of Vierordt's principle is seen in an experiment by J.-J. Van Biervliet (Le toucher et le sens musculaire, *L'Anneé Psychol.*, 1907, **13**, 114–121) who found a rough agreement between the mobility of the foreheads of 20 *O*s and the two-point discrimination; he also detected an increase in muscular tension in parts of the body which were receiving 'passive' stimulation.

[11] R. H. Lotze, *Medicinische Psychologie*, 1852, 398. A theory such as Lotze's really belongs to the long line of associationistic theories (extending from Berkeley through Wundt to Stout and others) according to which kinaesthetic (as well as visual) imagery is added to touch to give space perception: Cf. J. Peterson, Local signs as orientation tendencies, *Psychol. Rev.*, 1926, **33**, 218–226.

scription of intensive and qualitative sums of cutaneous *Mitempfindungen*. Movement merely brings about a successive order of sensory patterns enhancing their difference and giving the idea of adjacency. Quite recently J. Peterson [12] referred to 'local signs as orientation tendencies' and wrote,

"As the arm contracts, elongates, bends this way and that, turns over somewhat on its longitudinal axis, and performs other avoiding movements under the influence of the stimulus, certain motor impulses again, as distinct from others, will necessarily change concurrently with changes in the sensory impulses which the movements resulting from them effect, and again we shall have a finer specialization of response to local stimulation; and so on, down to the limits of mobility. Evidently the greater the mobility of any of the limbs and of their local surfaces the finer the coördinations will be which are set up, or the greater the discriminability as to place of stimulation."

Hardly any experimental evidence has been reported regarding the effect on spatial judgments of the responses occurring at time of stimulation.[13] E. Schimpf [14] reports a case in which judgments of the two-point threshold were very poor on an ankylotic knee. W. O. Krohn [15] also reports a case of defective judgment of two-point threshold on a forearm which had just been removed from a cast in which it had remained three months. Goldstein and Gelb [16] describe the case of a patient who had received a brain injury and had apparently been deprived of all visual memory and visual imagery. When the patient closed his eyes and remained motionless he was unable to recognize the location of tactual sensations on any part of the body. The process by which the patient localized tactual sensations is reported by Goldstein

[12] *Loc. cit.*

[13] Most experimental studies of localizing response have been concerned with the various methods of *indicating* the location of a spot after it has been stimulated. These methods include various ways of pointing to the spot, with or without tactual exploration, with flexor or extensor arm movements; sometimes they include the focussing of the eyes on the spot; and sometimes verbal report. A good summary of these studies as well as a re-examination of all the principal methods is presented by L. E. Cole, The localization of tactual space, a study of average and constant errors under different types of localization, *Genet. Psychol. Monog.*, 1929, 5, 339–450.

[14] E. Schimpf, *Raumsinn der unteren Extremität bei Anchylose des Kniegelenks*, 1882. Cited by V. Henri, *op. cit.*, 27.

[15] W. O. Krohn, Sensation-areas and movement, *Psychol. Rev.*, 1894, 1, 280–1.

[16] K. Goldstein & A. Gelb, Psychologische Analyses hirnpathologischer Fälle auf Grund von Untersuchungen Hirnverletzter. II. Über den Einfluss des vollständigen Verlustes des optischen Vorstellungsvermögens auf das taktile Erkennen, *Zsch. f. Psychol.*, 1920, 83, 1–94.

and Gelb to be similar to that used by young children. With the eyes closed, the patient began to stir all parts of the body, while a continuous tactual stimulation was presented. A brief stimulation was never localizable. Gradually the general bodily movements became restricted to the member stimulated. This member continually exhibited slight quivering and rotary movements ('*Tastzuckungen und -bewegungen*'). Meanwhile the patient would point with a finger to the general neighborhood of the stimulated spot (much in the same manner that one brings forward a hand or finger to a region which is being tickled). The quivering movement was continued even while the exploring finger moved about the skin (if the local movement stopped the exploring finger would pause in mid-air while the patient lost all idea of locating the spot on the skin). Goldstein and Gelb assert that the localizing process exhibited by their patient represents only the initial step—that of 'automatic localization'—in the development of the normal individual's perception. They claim that early in childhood the normal individual ceases to make the general movements or quivering responses. A further step in development is soon reached which consists of attention to visual imagery and to patterns of touch quality.[17] C. Spearman [18] seeks to show that tactual orientation depends upon a 'progressive chain of partial factors' involving both the stimulated and the exploring members. One factor is that of posture—the angles of the joints cause a constant error of localization toward the midpoint of the arc through which a member is allowed by its joints to move. Another factor is movement in the stimulated member. Spearman reports that when the forearm moved upward a distance of 1 cm. to the stimulus-point a smaller constant error resulted than when the member was motionless during stimulation; the average variable error remained the same. This effect is greater when the entire arm moves upward to the stimulus than when only a finger moves while the upper arm remains motionless.

[17] K. Goldstein has recently described somewhat related phenomena: Über Täuschungen des Tastsinnes unter pathologischen Umständen, *ibid.*, 1933, **129**, 282–290.

[18] C. Spearman, Die Normaltäuschungen in der Lagewahrnehmung, *Psychol. Stud.*, 1906, **1**, 388–493.

The foregoing review is offered for the purpose of showing that there is an extensive interest in the occurrence of localizing response at the time of stimulation, and that the interest has led mostly to theoretical discussions and only slightly to experimental investigation. The problem invites further empirical study.

II. Procedure

In the present study the influence on tactual localization of two fairly simple types of arm movement was tested. In both types of movement the right forearm swung through a horizontal arc of 90 degrees between the limits of a sagittal position straight forward from the side of the body and a transverse position across the stomach. One movement was an inward swing from the sagittal to the transverse position; the other movement was an outward swing from transverse to sagittal. The upper arm hung straight down; the elbow was held near the side of the body and rested on a smoothly turning pivot; the forearm was held horizontally with the palmar side up; the palmar side rotated slightly toward the stomach as the forearm swung from sagittal to transverse (this slight pronation is anatomically necessary); the fingers gripped a hand rest which was connected by a bar to the pivot supporting the elbow. The observers assumed a position half standing and half sitting against the edge of a table: in this position with the trunk bent slightly forward the upper arm could hang perpendicularly and the simplest horizontal swing of the forearm could be made. The palmar surface of the forearm was stimulated with a pressure point which consisted of a blunt wooden point for observers Br, Ca, Ga, Gi and Sc, and consisted of a stiff hair for the other observers. The pressure point was attached to an adjustable armature which extended from an electromagnet. The electromagnet was supported above and beyond the fingers by a rod which connected it with the same bar to which the hand rest was attached. Thus, when the forearm swung through the horizontal arc, the pivot under the elbow, the hand rest, and the tactual stimulus all swung together with it. A fixed electric contact placed midway between the limits of the 90 degree arc allowed a momentary actuation of the magnet and a quick downward thrust of the pressure point on the arm. Since the pressure point turned with the arm the stimulation did not brush across the skin but only touched a single spot. The armature attached to the pressure point was adjustable so that the pressure point could be moved anywhere over the upper surface of the forearm. But due to the slight rotation of the palmar surface as the arm swung between the limiting positions the point of contact could only be approximately determined before stimulation. Therefore the palmar surface was stamped with a large cross-section pattern (aligned with the longitudinal and transverse axes of the arm), and the pressure point was touched with burnt cork or ink just before the arm swung through the arc, so that when the pressure point touched the arm it left a small dot on the cross-section pattern. The observers held a stylus with a blunt wooden point or a projecting hair in the left hand, and in conformity with Weber's 'second method,' a tactual exploration was made of the right forearm until the stylus was brought to rest on a spot judged to be the place that was stimulated. The locations of the judged spot and the stimulated spot were immediately recorded on a duplicate cross-section pattern stamped on paper. The distance and direction of the judged spot from the stimulated spot was measured (in mm.) in terms of the distal-proximal (longitudinal) direction and the radial-ulnar (transverse) direction on the arm.

III. Results

Two series of experiments were made. In the first series, when the forearm was stimulated as it swung in, the forearm remained in (across the stomach) while the localization judgment was made; and when the forearm was stimulated as it swung out, the forearm remained out (straight forward) while the localization occurred. For each observer the constant errors of localization for all the inward swings and the constant errors for all the outward swings were calculated. A constant error represents the average of the algebraic sum of the errors for one axis. Thus for each observer there is a longitudinal constant error and a transverse constant error for the inward swings, and likewise two such constant errors for the outward swings. *All figures are stated in millimeters.* Each observer gave 50 judgments for each type of swing. The results of the first series are shown in Table I.

TABLE I

Showing **constant errors**—in bold face type and showing (average errors)—in parentheses

Stimulation as arm SWINGS OUT
Localization with arm STRAIGHT FORWARD

Observer	Distal	Proximal	Radial	Ulnar
Br		**10.3** (19.3)		**21.7** (21.9)
Ca	**29.4** (30.5)			**1.6** (7.2)
Ga	**23.0** (24.6)			**5.8** (7.2)
Gi	**9.7** (12.0)			**4.3** (5.4)
Sc	**5.6** (10.4)			**3.2** (5.2)
Average of C.E.'s	**13.5**	**2.1**		**9.3**
σav.	6.21	1.96		4.44

Stimulation as arm SWINGS IN
Localization with arm ACROSS STOMACH

Br		7.2 (14.6)		**13.0** (14.0)
Ca	**30.8** (31.0)		**3.5** (6.9)	
Ga	**11.8** (18.5)		**1.0** (6.8)	
Gi	**8.1** (12.6)			**4.2** (5.1)
Sc	**7.4** (10.6)		**1.8** (6.3)	
Average of C.E.'s	**11.6**	**1.4**	**1.3**	**3.4**
σav.	6.79	1.34	.58	.08

TABLE II

Showing **constant errors**—in bold face type and showing (average errors)—in parentheses

Stimulation as arm SWINGS OUT
Localization with arm ACROSS STOMACH

Observer	Distal	Proximal	Radial	Ulnar
An	**3.7** (14.8)			**3.4** (4.1)
Ba	**9.2** (10.0)		**.4** (2.4)	
Br		**8.4** (15.5)		**17.4** (17.5)
Co	**63.2**		**1.2** (6.0)	
Du		**6.4** (12.8)		**2.8** (4.4)
Fl	**36.4** (36.4)		**4.4** (6.8)	
Fo	**.3** (15.1)			**11.1** (11.8)
Fr	**21.6** (25.6)			**22.0** (22.0)
Ga	**19.1** (22.1)			**4.4** (6.9)
Gi	**6.3** (11.9)			**6.5** (8.4)
Ja	**11.6** (11.6)		**8.4** (10.0)	
Mi	**2.2** (11.1)			**6.6** (6.6)
Sc	**9.2** (12.3)			**2.2** (5.8)
Wa		**.8** (16.8)		**.8** (7.2)
Wh	**17.6** (18.4)			**1.2** (4.4)
Wi	**3.0** (31.4)		—(6.4)—	
Wn	**40.8** (40.8)		**16.8** (16.8)	
Wo	**10.1** (14.9)			**1.6** (3.2)
Average of C.E.'s	**14.1**	**.9**	**1.7**	**4.4**
σav.	5.06	.54	1.03	1.55

Stimulation as arm SWINGS IN
Localization with arm STRAIGHT FORWARD

An		**.4** (10.7)		**4.4** (6.6)
Ba	**3.6** (11.6)			**.4** (4.4)
Br		**8.0** (12.9)		**12.2** (12.4)
Co	**30.8** (33.9)			**4.4** (6.0)
Du	—(7.2)—		**1.2** (5.2)	
Fl	**12.0** (20.8)		**.4** (5.2)	
Fo		**16.4** (23.6)		**6.4** (8.8)
Fr	**17.2** (23.6)			**14.4** (14.4)
Ga	**17.0** (18.0)			**1.8** (6.2)
Gi	**2.0** (8.5)			**.3** (4.9)
Ja	**19.2** (21.3)		**6.2** (10.3)	
Mi	**8.4** (10.8)			**2.4** (5.6)
Sc	**9.6** (13.1)			**.4** (5.4)
Wa	**9.6** (18.4)			**3.6** (6.8)
Wh	**6.4** (9.4)			**2.4** (5.4)
Wi	**4.4** (14.0)		**3.3** (7.7)	
Wn	**24.8** (26.4)		**7.6** (13.2)	
Wo	**29.4** (29.4)		**6.4** (6.4)	
Average of C.E.'s	**10.8**	**1.4**	**1.4**	**3.0**
σav.	3.34	.94	.61	1.13

In the second series, when the forearm was stimulated as it swung in, the forearm immediately swung out again through the 90 degree arc to the starting position (straight forward) in which position the judgment of localization took place; likewise, when the forearm was stimulated as it swung out, the forearm immediately swung in again to the starting position (across the stomach) where the localization occurred. Thus in the two series the position of the forearm during localization is reversed in its relation to the swing of the arm during stimulation so as to cancel or minimize the effect of posture [19] or the effect of the direction from which the pointing arm arrived.[20] Again the two constant errors of localization for each type of swing for each observer was calculated. Observers Br, Ga, Gi and Sc gave 50 judgments for each type of swing; the other observers gave 12 to 14 judgments for each type. The results of the second series are shown in Table II.

In the two experimental series there is more agreement between the results of the same kind of arm movement during stimulation than there is between the results for the same position of the arm during localization. The ulnar constant error is somewhat reduced when the arm swings in (*i.e.*, in the ulnar direction). In view of the size of the deviations the significance of these results will depend upon the study of other movements, such as longitudinal to-and-fro movements during stimulation. The present investigation offers empirical observation upon a situation which has been discussed for a century.

[19] *Cf.* C. Spearman, *op. cit.*

[20] *Cf.* W. B. Pillsbury, Some questions of the cutaneous sensibility, *Amer. J. Psychol.*, 1895, **7**, 54.

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THE INCONSTANCY OF THE WEBER–FECHNER 'CONSTANT' FOR AUDITION

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That the Weber-Fechner law as originally formulated has some serious defects has long been recognized by workers in the field of psycho-physics. One of its major limitations is the fact that the ratio by which the intensity of the stimulus must be increased or decreased in order for the change to be discernable is not constant. Most authors hold that the fraction is a constant over a certain range of intensities; but there seems to be little agreement as to its exact size. Wundt (1901, p. 32) gave the fraction for audition as one-third, the same as for pressure, while Dashiell (1928, p. 115) puts the value at one-fifth with limits of one third and one eighth. Most elementary texts give some such figure to represent the value of K (the ratio of the increase in stimulus intensity to the original intensity). The extent to which K varies with the subject, and with the loudness, frequency level, and temporal relationships of the tones is given but little attention, if it is even mentioned. That these variables may influence the size of K has long been recognized but the working out of their specific relationships has only just begun.

The present study is concerned with variations of K with different subjects and with various intensity levels.

Apparatus

The apparatus used is diagramed in Fig. 1. The source of sound is the 800 cycle, type 210, vacuum tube oscillator shown at the left in the diagram. The condenser C at the terminals of the oscillator acts as a filter to purify the wave form of much of the higher harmonic content present in the oscillatory circuit, and reduces somewhat the effect of transients due to opening and closing the key. With the exception of a noticeable ac hum the tone is quite pure. The output of the oscillator is impressed across a maximum resistance of 1300 ohms. By means of the 1,000 ohm potentiometer any percentage of the oscillator's output can be impressed on the primary of the 5 : 1 impedance-matching transformer. The sound power is changed by depressing or

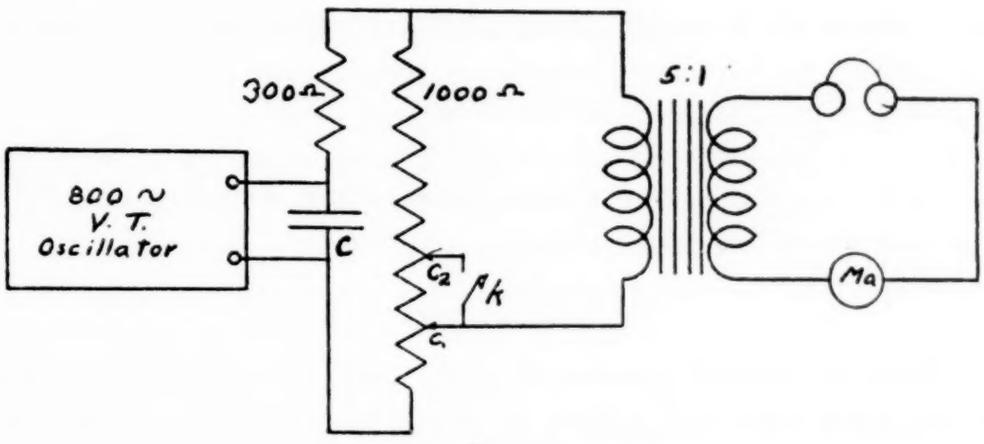

FIG. 1

releasing the key *k*. The head set is connected to the rest of the apparatus by the step-down transformer. Thus, only alternating current energy flows in the 'phone circuit. The current is read by a 0–1 ma. rectifier type a.c. ammeter (Ma in the diagram). Knowing that sound power is proportional to the 'phone-current squared, *relative* sound powers can easily be determined. Since the apparatus was not calibrated it was impossible to say exactly what the acoustic energy was in watts, and hence the sensation level in decibels. However for a determination of the value of K, this data was not required since the relative acoustic energy was known.

PROCEDURE

The method of minimal change was used in determining the just noticeable differences for each subject. The subject was seated comfortably in a chair out of sight of the experimenter and far enough away that the sound of manipulating the apparatus would not affect his judgments. The contact C_1 was set so as to give the desired acoustic power and contact C_2 was far enough from C_1, so that when the key *k* was depressed or released a very noticeable change in sound intensity occurred. While depressing and releasing the key approximately three times per second, the distance between contacts C_1 and C_2 (and consequently the intensity difference between the two tones) was decreased, until the subject signified that the change was no longer evident. The exact point at which the difference could be barely heard was determined and the readings for the two different intensities were recorded. The smallest current was recorded as I_1 (the current with key *k* depressed) and the stronger current as I_2 (the current with the key released). Readings were similarly taken by starting the differences below the limen and then gradually increasing the difference until it was just barely perceived by the subject.

Several checks were made to determine whether or not the subject was responding to the intensity differences or to some other cues supplied by the experimenter in the manipulation of the apparatus. One method consisted in tapping the key regularly, sometimes connected in circuit and sometimes not functioning in the circuit at all. If the subject could state correctly when the key was actually in circuit, it was assumed that he was responding to the intensity differences. Another check was to either increase or decrease the intensity of the tone, and ask the subject to indicate whether the change was an increase or decrease. The last method consisted in changing the sound power at irregular intervals and requiring the subject to indicate the exact times the changes occurred. All of these checks indicated that the subjects were actually responding to the change in sound intensities and not to any extraneous cues.

Values of K were computed according to the formula

$$K = \frac{I_2^2 - I_1^2}{I_1^2}$$

when I_1^2 equals 'phone power with switch k closed and I_2^2 is the 'phone power with k open.

Results

Relative sound powers of .010, .040, .160, .360, .640, and 1.000 were used and values of K for each subject at each intensity were computed. The results of these calculations are given in Table I. It will be noted that the individual differences are rather great. The percentage deviation (A.D./Mean) ranges from 31 for .160 to 53 for .010. The extreme intensities show the greater variability. The change in K with sound power is shown graphically in Fig. 2. This curve is based on the average of all 12 subjects. This graph shows a consistent increase in the value of K with decrease in

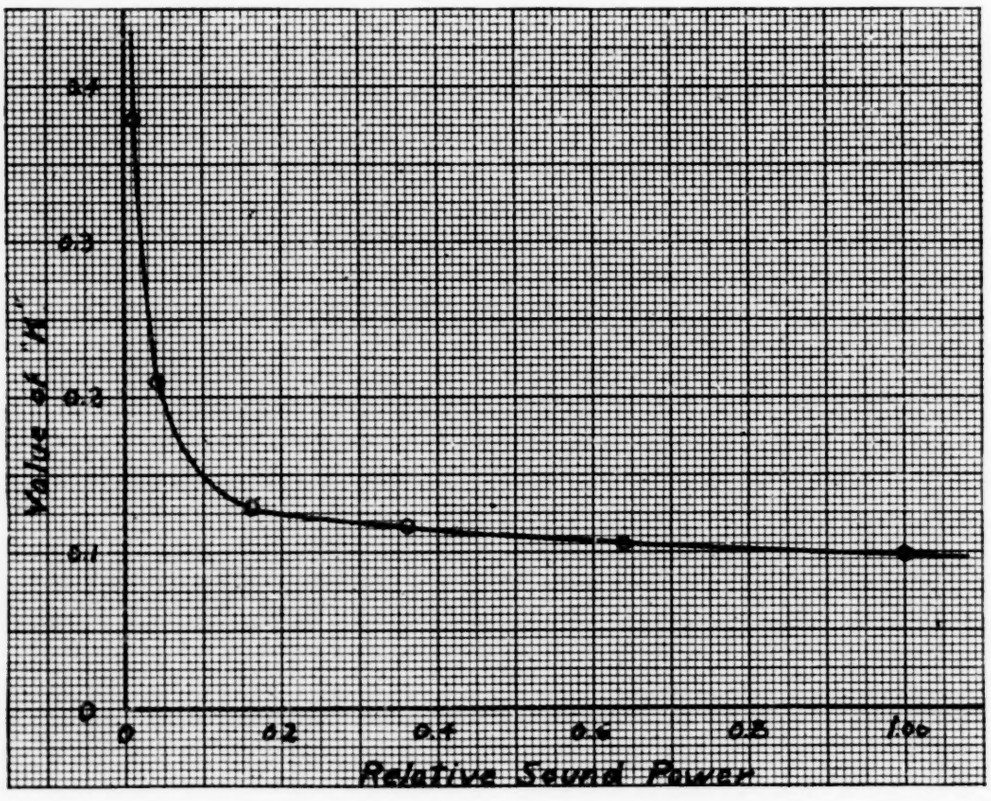

Fig. 2. Showing variation of the 'constant,' K, against sound volume. From an avg. of 12 subjects.

intensity of the sounds. Unfortunately our apparatus was not calibrated; consequently, absolute sound intensities cannot be given. The values listed are for sounds slightly above the threshold to intensities which became uncomfortable to the listener after a few minutes.

Within these limits the average values of K vary from .10 for very loud sounds, to .38 at relatively low acoustic powers. It will also be seen from Table I that there is a range of as

TABLE I

VALUES OF 'K' FOR 12 SUBJECTS AT SIX DIFFERENT INTENSITY LEVELS

Relative Sound Power	.010	.040	.160	.360	.640	1.000	Means
Subject							
I	.18	.11	.05	.07	.05	.06	.09
II	—	.23	.16	.19	.20	.21	—
III	.25	.11	.11	.11	.08	.06	.12
IV	.25	.11	.11	.11	.08	.09	.13
V	.60	.38	.17	.15	.11	.13	.26
VI	1.00	.38	.23	.19	.24	.23	.38
VII	.60	.38	.17	.19	.17	.11	.27
VIII	.25	.11	.05	.03	.05	.04	.09
IX	.25	.23	.11	.11	.08	.09	.15
X	.25	.23	.17	.11	.08	.06	.15
XI	.25	.11	.11	.07	.08	.06	.15
XII	.25	.11	.11	.07	.05	.06	.11
Total	4.13	2.49	1.55	1.40	1.27	1.20	1.90
Mean	.38	.21	.13	.12	.11	.10	.17
A.D.	.20	.10	.04	.04	.05	.05	.07
$\frac{\text{A.D.}}{\text{Mean}}$	.53	.48	.31	.33	.45	.50	.41

much as .82 between the values of K for different subjects at the same sound intensity. The range in average K's for different subjects is from .09 to .38. This indicates that the differences between individuals are just as great as the differences resulting from varying intensities of sound, within the limits here used.

These results may be compared with those quoted by Fletcher (1929, p. 148) when he says, "For sensation levels above 50 db, the fractional change in the intensity which is just perceptible is between 5 percent and 10 percent. At a sensation level of 10 db the fractional increase must be 73 percent to be perceptible." For a frequency of 1024 (the closest

to our value of those used) Riesz (1928) gives values of .14 for a 'sensation level' of 60 decibels, .40 for 20 decibels, and 1.00 for 5 decibles. The range of values given by Knudsen (1923) seems to be from about .10 to almost .30 for a frequency of 1000. Riesz' data represents averages of the results of 12 persons, but no measures on separate individuals are given, while Knudsen's data are derived from measures on himself and one other person only.

Macdonald and Allen (1930) using a frequency of 180 obtained results comparable to our own but presumably made three series of measurements on only one person.

In addition to differences between individuals and variations due to different intensity levels of sound, there are several other factors which influence the value of K. One of these is the frequency of the tones. Knudsen (1923, p. 95) tested 19 individual's ears and concluded "although the individual curves differ appreciably and in many cases show that the Fechner ratio depends upon the pitch of the tones, the average curve indicates that from 100 d.v. to 3200 d.v. the Fechner ratio is almost constant, provided that the tones have the same loudness. The value of $\Delta E/E$ [K] over the above-named range is roughly 0.10." However Riesz (1928) shows considerable variation in K with changes in frequency level of the sound. He used tones 5 decibels, 20 decibels, and 60 decibels above the threshold and found that for the first intensity the differential sensitivity of the subjects was much less (K was larger) at lower frequency levels (around 64 cycles) than at higher ones (*i.e.* 1024 cycles); and that the differential sensitivity diminished at frequencies higher than 2048 cycles. Much less variation in differential sensitivity was found at the higher intensities with changes in frequency level. For tones 60 decibels above the threshold the ratio K, or $\Delta E/E$ (Riesz notation) remains practically constant over the entire range of frequencies used. The change in value of K with change in frequency seems to be limited to the lower intensities.

Biddulph (1933, p. 47) summarizes his work on the differential pitch sensitivity of the ear as follows: "Differential

pitch sensitivity is found to be a function of two variables describing the sound—the frequency of the tone and its sensation level. . . . Were the Weber-Fechner law a true statement of fact throughout the audible frequency range, these curves would be straight lines parallel to the axis of abcissæ. Above a frequency of 800 cycles the curves are approximately horizontal; below this value the differential pitch sensitivity rises rapidly but in such a way that the absolute detectable frequency difference remains constant. These data indicate the ear to be extremely sensitive to frequency differences; the human ear, for example, can distinguish a frequency difference of 0.3 percent in a 1,000 cycle tone at a sensation level 40 db above the threshold of audibility, or expressed in musical notation it distinguishes a difference of approximately 1/16 semi-tone in the tone two octaves above middle C on the piano." It will be noted that this ratio is extremely low. Biddulph does not indicate whether the figure was derived from one or more subjects and in view of the great individual differences found in the present study, it is possible that individual subjects may have sensitivities this great.

The temporal relationships of the compared tones also influence the size of the differential ratio (K). Riesz (1928) studied the relationship between the duration of the compared tones and the value of K. The duration of the tones varied from 0.1 to 5.0 seconds. His data indicate that the smallest proportional change in intensity can be descriminated (value of K is smallest) when the compared tones last 0.3 second. If the tone duration is shorter or longer than this the proportional change in intensity must be greater in order for the subjects to observe a change in loudness.

Instead of varying the duration of the tones as did Riesz, Knudsen (1923) varied the time interval between the tones and determined in what way this influenced the value of K. Eleven time intervals ranging from 0.4 to 10.0 seconds were used and maximum sensitivity was found for the 0.4 second interval. Knudsen's data show a constant increase in the size of K as the interval between the tones is increased.

Summary and Conclusions

The data presented in this paper and a review of previous studies indicate that the value of the Weber-Fechner 'Constant' K $(\Delta E/E)$ for audition is not in reality a constant, but is a function of:

1. The absolute intensities of the tones compared. The range of values of K obtained in the present study for different intensities is from .10 to .38 (averages of 12 subjects).

2. The subjects on whom the measurements are made. Value of K range from .09 to .38 for different subjects (averages of 6 different intensities).

3. The frequency level of the tones. The variations in value of K with change in the frequency of the tones compared seems to be limited to the lower frequencies (Knudsen, 1923) (Biddulph, 1933).

4. The time duration of the tones. The optimum duration of the tones is found to be 0.3 (Riesz, 1928).

5. The interval between the tones. The shortest time interval used (0.4 seconds) was found to result in better discrimination (lower value of K) than any longer interval up to 10 seconds (Knudsen, 1923).

(Manuscript received November 7, 1933)

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THE DEVELOPMENT OF TOLERANCE FOR CIGARETTES[1]

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The effect of the use of cigarettes on human behavior depends partly upon the amount of congenital or acquired tolerance which an individual has inherited or acquired for nicotine. Although this fact would be generally accepted by both physiologists and psychologists, experimental evidence of either the presence or development of tolerance is lacking, as far as human subjects are concerned. However, a well defined tolerance for nicotine was obtained by Esser (1) when he administered large doeses of nicotine to dogs and rabbits by means of hypodermic injections and by mouth. Hatcher (2) also observed that tolerance could be induced in rabbits and that the rate of development was influenced by the size of the dosage. In both of these studies with laboratory animals, tolerance was found to be limited, but once it was gained, it was retained for some time.

If one may assume that certain behavior changes such as those used reflects the presence or development of tolerance, there seemed to us to be no reason why such experiments should not be carried out with human subjects, employing inhaled tobacco smoke in the place of injected nicotine. It would thus be possible to determine (1) the rate at which tolerance for tobacco may be developed, (2) the extent to which it may be developed, (3) the effect of the size of the dosage on the rate of development, (4) the duration of a developed tolerance, and (5) the influence of age, size and sex on such a development. The following exploratory investigation represents an attempt to check this method for cigarettes.

[1] The investigation upon which this article is based was supported by a grant from the Heckscher Foundation for the Advancement of Research established by August Heckscher at Cornell University.

Our first problem was to decide which aspect of human behavior we could use as a control. After some consideration, both motor and secretory activities which had been found by preliminary tests to be definitely influenced by tobacco were selected for observation and measurement. On the motor side it was found that steadiness was definitely affected by inhaled cigarettes and since this aspect of behavior is capable of exact measurement, it was selected for one control in the experiment. A device was constructed by which variation in steadiness could be recorded. This device consisted of a strip of copper sheeting through which was pierced a series of holes 11.5 mm. in diameter. This strip was mounted on an adjustable support five feet from the floor. A binding post was provided on the lower end of the sheeting permitting a circuit with a battery, an electric counter, and a copper stylus 6 mm. in diameter which was mounted on a hand grip and attached to a flexible cord. Whenever the stylus came in contact with the sheeting the counter recorded the contact.

The subject was instructed to stand erect on a designated line and, holding the stylus at arm's length, insert it on a given signal into that hole on the sheeting most convenient for his height. His problem was to hold the stylus as steady as possible for one minute so as to avoid contacting the sides of the hole. Each contact counted one error and the number of errors per minute indicated the degree of his steadiness. At the end of the minute he withdrew the stylus and rested for three minutes before repeating the process. This relatively long rest period was introduced to remove the possibility of fatigue entering as a disturbing factor.

After a preliminary practice period in which the subject reduced his errors to a minimum, the real test with cigarettes began. A series of three trials at the beginning of the day's run served as a control record. The subject, who had never smoked before, was then handed a standard cigarette and told to inhale as much of the smoke as he could. The smoking of the cigarette was so planned that it would continue through three consecutive rest periods, a total of nine minutes. During the intervening tests the cigarette was laid aside while the record was made. After the smoking period the tests for steadiness were continued every fourth minute as long as the subject showed any disturbance from his experience. This procedure was repeated at the same hour each day until the experiment was concluded. The subject smoked just the one cigarette per day under laboratory control.

Although some experimenters with smoking have cautioned their subjects to avoid inhaling the smoke, it seemed to us desirable to have them do so. After all, it is only inhaled smoke that effects behavior to any measurable extent and it is impossible to prevent some smoke entering the lungs even when the subject is cautioned to be careful. For our purpose it was particularly desirable to obtain a consistently measurable difference and to develop a dependable standardized dosage.

Three mature college men were used as subjects who were not smokers. While there was some difference in the rate of the development of tolerance, as shown in their records, the same general characteristics are found in each record. One

record is presented for analysis. The record of the first six cigarettes was eliminated from this graph because the subject experienced some difficulty in learning to inhale the smoke and did not succeed in getting the standard amount into his lungs at first.

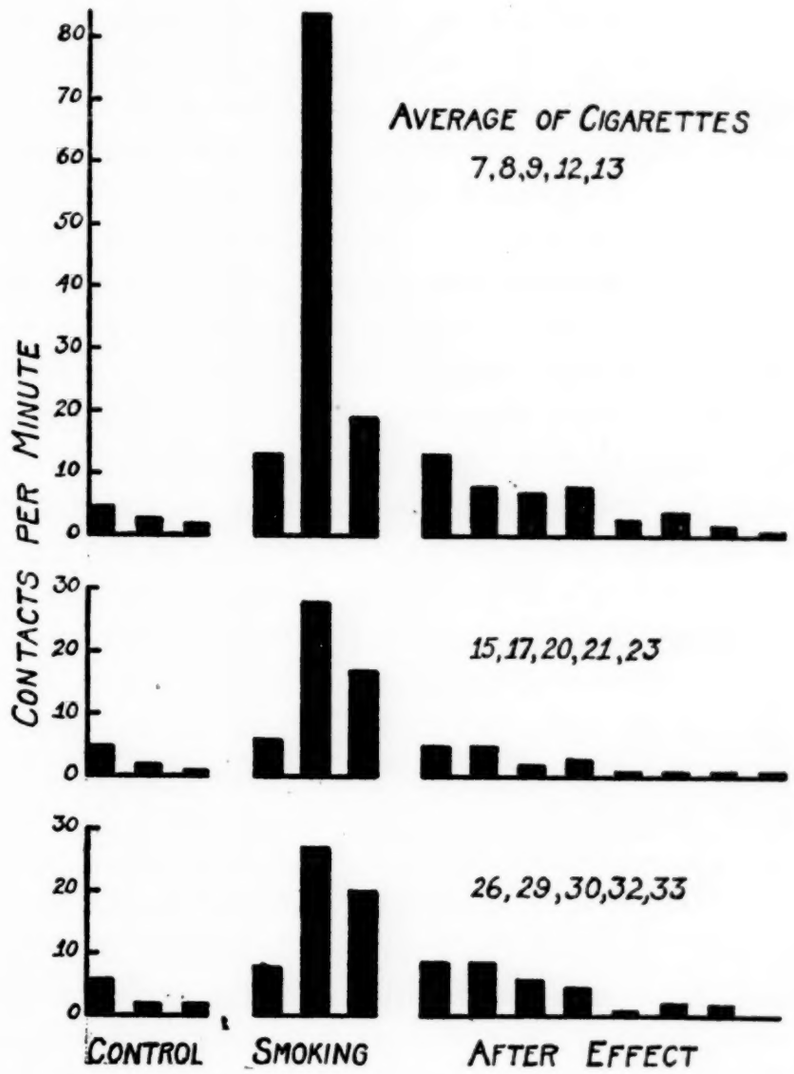

FIG. 1. The development of tolerance for cigarettes. Showing the extent and rate at which unsteadiness caused by inhaling cigarette smoke may be reduced by daily repetition of the dosage.

In the first graph in the figure the first three columns represent the control records, the next three the records made during the smoking period and the remaining columns represent the records made at intervals subsequent to smoking. This graph represents an average of five cigarettes taken at the beginning of the experiment. The second graphs represent cigarettes

15–23 and the third, cigarettes 26–33. Whereas this individual made from 2–5 errors before smoking, it will be observed that the number increases to 80 before he had finished his cigarette. This disturbance continues about 30 minutes after the smoking is finished. The greatest degree of unsteadiness is reached during the smoking period, then the curve drops rapidly. As the subject continues on his diet of one cigarette per day, the extent as well as the duration of unsteadiness is gradually reduced. After the third week, however, no further progress was made even though the experiment continued for 90 days. In his case a limit of tolerance was reached beyond which no further progress could be made. The records of the other subjects are similar in that the limit of tolerance appeared rather early in the experiment. In general by the 20–25th cigarette, the effect was reduced as low as we were able to reduce it. Under the conditions of our experiment the human organism seemed to be incabaple of complete tolerance to cigarettes, when most of the smoke was inhaled, even though only one cigarette was smoked per day.

In an earlier study (**3**) of the effect of cigarette smoking on the salivary secretion of smokers and non-smokers, we found that smoking was followed by inhibition of secretion in the case of non-smokers and by excitation of secretion in the case of smokers. Using the same technique for collectiong and measuring the parotid secretion, we outlined a procedure whereby the effect of cigarettes on secretion could be charted each day from the time the subject smoked his first cigarette until the smoking of subsequent cigarettes caused no further change, as in the case of the steadiness test. Here again the subjects had never smoked before and during the experiment smoked one cigarette per day. The rate of secretion before, during and after the smoking was carefully measured in terms of hundredths of a cc. The time taken for the smoking of the cigarette was the same as that employed in the steadiness test.

The graphs in Fig. 2 show the rate of secretion following the smoking of the 1st, 14th and 28th cigarettes. It will be observed that whereas the first cigarette causes partial inhibi-

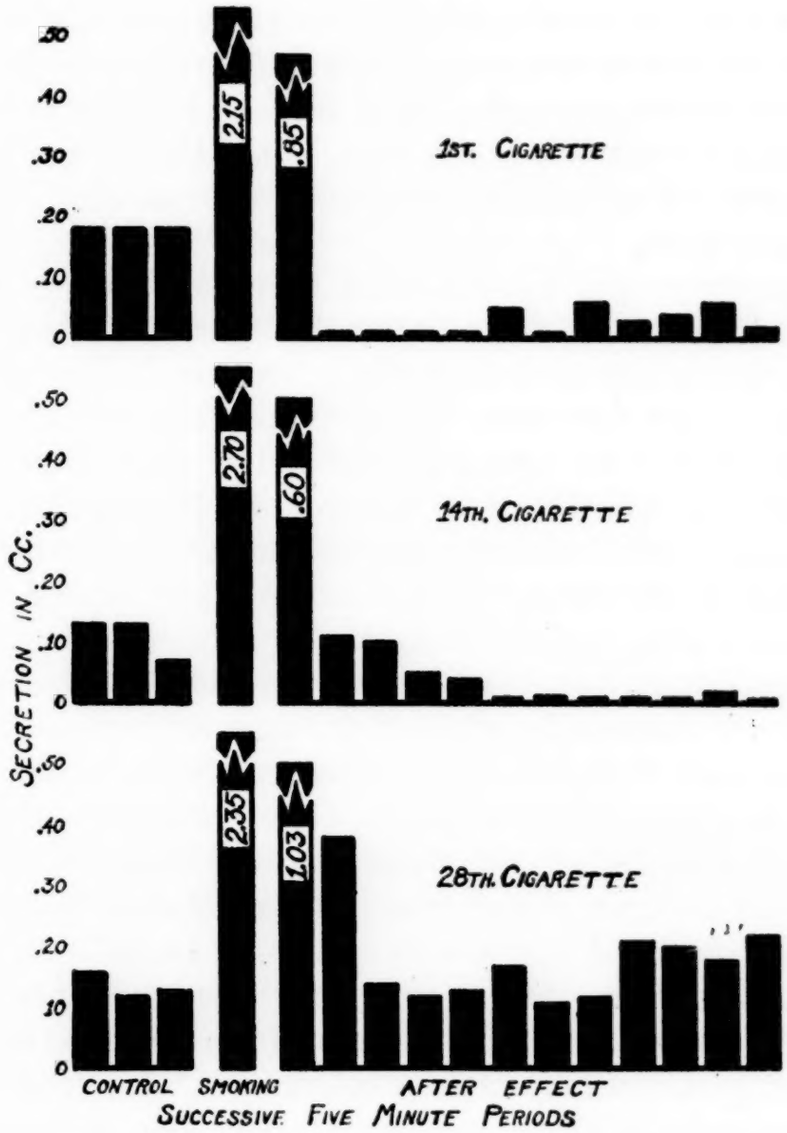

FIG. 2. The development of tolerance for cigarettes. Showing the extent and rate at which the initial inhibition of secretion caused by inhaling the smoke of a cigarette may be reduced by daily repetition.

tion of secretion for at least one hour, this inhibition is gradually reduced in extent and duration as the experiment proceeds until the smoking of a cigarette is followed by an increase rather than a decrease in the rate of secretion.

The effect of the development of tolerance is shown, in this study, in the gradual increase in secretion, or the reduction of inhibition. In an earlier study (4) of the development of tolerance for caffeinated beverages, tolerance was characterized by a reduction of excitation. In the motor experiment

the effect of the development of tolerance was shown in the gradual reduction of unsteadiness. In the secretion experiment the development seemed to be complete whereas in the motor experiment it was not complete. The secretory processes seem to be capable of greater adaptability than the motor processes.

After the partial tolerance had been developed to a point beyond which we seemed to be unable to make further reductions in unsteadiness, an attempt was made to determine the duration of such tolerance. The subjects discontinued smoking daily, but were tested at the end of each week. This procedure was continued for six weeks. During this period there was a partial breakdown of tolerance. At the end of the first week of abstinence there was a marked increase in unsteadiness during and following the smoking of the cigarette. Both the extent and duration of unsteadiness was greater again as the subjects lengthened the interval between smokes from one day to seven days. However, some tolerance remained at the end of that time. The subjects then discontinued smoking entirely for three months. Tests made at the end of this period revealed practically no change in tolerance from that observed at the end of the six weeks' period. It would appear from these results that discontinuance of the habit of inhaling most of the smoke from a cigarette is followed by a partial reduction in tolerance. This reduction is completed within a few weeks and after twelve weeks the reaction shows little change. No subject in our experiment ever returned to the extreme unsteadiness which characterized his initial experiences with cigarettes. No check was made of the duration of tolerance for secretory activity.

Incidentally it should be observed that the subjects did not create a 'taste' for cigarettes. The smoking at the beginning was somewhat of an ordeal to a novice and continued to be so throughout the tests. The fact that none of the subjects had any desire to take up cigarettes after the research was completed may afford a clue to those who would block the development of the cigarette habit. By urging a youngster to inhale deeply most of the smoke of his first few cigarettes one

might definitely block any further desire to smoke. This would seem to be about as effective as swallowing chewing tobacco has been in discouraging the development of that habit.

From the point of view of the application of these results, it should be emphasized that very few individuals ever inhale as much of the smoke of a cigarette as was inhaled in this experiment. In fact one might smoke a package per day and still not inhale nearly as much as was done here. It seems quite logical to assume that the average individual might well develop sufficient tolerance so that no unsteadiness or secretory changes would characterize a moderate smoking schedule. In our preliminary survey we found some individuals who were smoking far beyond their capacity to develop tolerance. These subjects exhibited extreme unsteadiness when they smoke at their normal rate and in their usual way, merely submitting to steadiness tests before and after smoking. Other smokers on the other hand actually made better records during and subsequent to smoking than they did before they smoked. These facts seem to emphasize the importance of each individual's paying some attention to his capacity to tolerate tobacco and arrange the extent of his consumption in accordance with that tolerance. If the individual had been smoking long enough to develop as much tolerance as his organism could stand and found by a steadiness test that during and subsequent to smoking he exhibited marked unsteadiness he might safely assume that he is inhaling too much nicotine. By changing the number of smokes or the mode of smoking he could then ultimately determine his maximum tolerance capacity.

Further research in this whole field of tolerance would seem to be desirable. At the present time we are engaged in a study of the effect of quantity and distribution of dosage on the rate of development of tolerance. The effect of age, size, and sex seem to be significant as far as congenital tolerance is concerned and they may show significant differences in tolerance development.

Summary

This study represents an attempt to determine the extent to which man may develop tolerance for cigarettes as far as progressive changes in motor or secretory activity may be assumed to represent fundamental changes of tolerance.

When most of the smoke from one cigarette was inhaled deeply by unhabituated subjects on successive days, definite changes in both motor and secretory activity occurred.

There was a marked but gradual reduction in both the extent and duration of unsteadiness for 20 to 30 days. Thereafter no further reduction could be attained. Approximately a 50 percent tolerance was achieved as far as motor activity was concerned.

The change in secretory activity was from definite inhibition following the smoking to at least the normal rate. The tolerance seemed to be complete as far as this function was concerned by the 28th day.

When the smoking was discontinued the partial tolerance, represented by a reduction in unsteadiness, began to disappear immediately but some tolerance was still apparent after a 3 months' interval.

Individual differences in both the degree of tolerance attainable and the rate of its development were observed.

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THE EFFECT OF SIGNAL FOR ERROR UPON MAZE LEARNING AND RETENTION

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The aims of this experiment were (1) to determine the effect upon the learning and retention of a stylus maze of giving an auditory signal for error during the learning, and (2) to compare the effect of the above signal with that of administering an electric shock for error, which we have reported in a previous study.[1] The auditory signal, it should be said at once, was a combined buzz and click produced, respectively, by a Porter inductorium and an electric counter whenever *S* touched the end of a cul-de-sac with the stylus.

The classification of the experiment within the general field of learning—its relationship to the problems of guidance, of incentives and motives, and of 'effect'—is dependent on various theoretical considerations whose discussion had in our opinion best follow rather than precede the presentation of the data. For the moment, therefore, it may suffice to note that the only investigation which closely resembles the present is that of Wang's [2] on verbal guidance in maze learning. In this study informing the *Ss* verbally of every error just after they had made it was found to improve learning according to the criteria of trials and errors though it did reduce final speed. Although an auditory signal for error may be regarded as functionally equivalent to verbal information thereof, Wang's experiment differs from ours in two important respects: (1) He gave his information during a period of from one to twelve trials only, after which *S* had to learn the maze without such aid, whereas we employed the auditory signal throughout.

[1] L. W. Crafts and R. W. Gilbert, The effect of punishment during learning upon retention, this JOURNAL, 1934, 17, 73–84.

[2] T. L. Wang, The influence of tuition in the acquisition of skill, *Psychol. Monog.*, 1925, 34, No. 154, 51 p.

(2) He did not measure the retention of his *Ss*. These differences are of course quite sufficient to justify further investigation as to the effect of signals for error upon learning. Other guidance experiments are not at all comparable with the present study, both because the tuition has been given for a prescribed number of trials only, and also because, in maze learning at least, it has been of the mechanical or manual type in which no errors are possible during the guided trials, or of a demonstrational sort, such as allowing *S* to see the maze before he practices upon it without vision.

With reference to the second aim of the experiment, to compare the effect of the auditory signal with that of an electric shock administered under precisely the same conditions, it need only be said that it should be of interest to see if the signal, which perhaps possesses more clearly an informative than it does an incentive or a punishment function, would be as effective a stimulus as an electric shock to which all three functions may be definitely attributed. The literature contains no examples of any comparisons of this particular sort, except possibly certain experiments of Tolman [3] and of Lorge [4] which will be described later.

Method

Subjects.—The *Ss* were twenty-five male and twenty-five female undergraduates at New York University. None had had any previous experience in maze learning. As a group they closely resembled in class of origin, college year and chronological age the groups in our earlier experiment with whom they will later be compared.

Apparatus.—The maze employed was the McGeoch and Melton [5] medium maze, constructed as described in our previous experiment. The stylus and its electrical connections were also as described therein, except that the latter were

[3] E. L. Tolman, C. S. Hall and E. P. Bretnall, A disproof of the law of effect and a substitution of the laws of emphasis, motivation and disruption, this Journal, 1932, **15**, 601–614.

[4] I. Lorge, The effect of intensified reward and intensified punishment, this Journal, 1933, **16**, 177–207.

[5] J. A. McGeoch and A. W. Melton, The comparative retention values of maze habits and of nonsense syllables, this Journal, 1929, **12**, 392–414.

so arranged as to produce, when *S* touched the end of a cul-de-sac with the stylus, not an electric shock but a buzzing sound from the inductorium and a click from an electric counter which had been placed in the circuit. These sounds, though of only slight intensity, were clear and distinct.

Procedure.—Except for this substitution of the sounds for the shock, the procedure was identical with that employed for the experimental group in our previous study. The criterion of learning was, as before, to traverse the maze two out of three successive trials without a 'contact' error, *i.e.* without going so far in a cul-de-sac as to come in contact with its farther end and so produce a sound. Other types of error were, so far as the criterion was concerned, entirely disregarded. After an interval of a week *S* relearned the maze to the same criterion but now no signal for error was given. Three males and two females who could not learn the maze within sixty minutes actual working time, or who had made no progress after about an hour in the laboratory, were classified as failed and their records discarded. The practice was of the massed type, since the maze was learned at one sitting with only very brief intervals between trials. As to the retention test, most *Ss* both expected such a test and tried to recall the maze during the interval, but the amount of such expectation and recall did not exceed that present in the former experiment.

In the results and discussion to follow 'signal group' will refer to the *Ss* of the present experiment. 'Shock group' will mean the group in our previous study who received an electric shock for every contact error, and 'control group' the group, also from the earlier study, who learned the maze in the usual manner without any signal or punishment at any time.

Results

The results for learning, given in Table 1, are as follows:

TABLE 1

LEARNING

Giving the averages and the S.D.'s for the signal group of the present experiment and the shock and control groups of the previous one, together with the reliability of the differences between them. N for each group is 50.

Criterion	Signal Group		Shock Group		Control Group		Diff./S.D.Diff.		
	Aver.	S.D.	Aver.	S.D.	Aver.	S.D.	Sig.-Cont.	Sh.-Cont.	Sig.-Sh.
Trials.....	15.5	8.58	17.2	8.40	23.3	8.74	4.51	3.52	0.93
Contact Errors.	120.9	63.36	127.4	70.74	220.0	114.10	5.47	4.89	0.48
Retracings....	162.7	94.06	135.6	110.97	218.7	139.23	2.36	3.30	1.51
Time.....	1501.7	675.70	1438.1	636.92	1804.0	797.00	2.43	2.54	1.25
Two Final Trials									
Entrance Errs.	1.02		0.92		0.52				
Retracings....	0.66		0.48		0.55				
Time.....	31.8		29.6		28.1				

The signal group, like the shock group, is superior to the control according to all criteria and is reliably so with respect to trials and contact errors. The differences between it and the shock group are, however, small and inconsistent. In its performance on the two final trials the signal group is slightly inferior to the others.

The results for retention are given in Table 2. In the re-

TABLE 2

RETENTION

Giving the recall, relearning and saving scores for the three groups.

Criterion	Signal Group		Shock Group		Control Group		Diff./S.D.Diff.		
	Aver.	S.D.	Aver.	S.D.	Aver.	S.D.	Sig.-Cont.	Sh.-Cont.	Sig.-Sh.
Recall									
Contact Errors.	3.4		4.9		5.3				
Retracings....	2.1		3.3		4.2				
Time.....	49.9		58.9		52.8				
Relearning									
Trials.....	5.6	3.87	5.7	6.49	7.6	6.62	1.84	1.49	0.02
Contact Errors.	15.9	15.25	16.6	19.78	20.7	21.55	1.29	1.00	0.19
Retracings....	7.4	10.82	7.3	8.21	15.3	25.00	1.95	2.14	1.51
Time.....	230.8	180.02	188.5	155.40	244.8	187.77	0.38	1.63	1.25
Per Cent Saving									
Trials.....	63.9		66.9		67.2				
Contact Errors.	86.8		86.9		90.6				
Retracings....	95.5		94.6		93.0				
Time.....	84.6		86.6		86.2				
Per Cent Saved per Trial, Error and Second of the Learning *									
Trials.....	4.12		3.89		2.98				
Contact Errors.	0.72		0.68		0.41				
Retracings....	0.59		0.64		0.45				
Time.....	0.056		0.061		0.049				

* These figures are obtained by dividing the per cent saving in trials, errors, etc. by the number of trials, errors, etc. required for the learning.

call score on the first trial of the relearning the signal group is slightly superior to both of the other groups according to all criteria. In relearning it is, like the shock group, superior in all respects to the control, though the difference in time is very slight and none of the differences is reliable; the differences between the signal and shock groups are, however, small

and inconsistent. In percent saved there are no consistent differences between any of the groups. In percent saved per trial, error and second of the original learning, however, both the signal and the shock groups are superior to the control, which means that a trial, an error, or a second of time contributed more to saving when made or spent under either of the two experimental conditions than it did under the control. There are, however, no consistent differences in this respect between the two experimental groups.

Results for the two sexes separately, though given in our earlier experiment, have in the present instance been omitted, the reasons being their statistical unreliability and their apparent irrelevance to the particular aims of the investigation.

Discussion

1. *Comparison of the Signal and Control Groups.*

(*A*) *Learning.*—That giving the *Ss* an auditory signal for error did facilitate their learning of the maze is clearly shown by the data of Table 1. The question inevitably arises, however, as to how this result is to be interpreted. Shall this facilitating influence be explained by attributing to the sound a guidance function, or by attributing to it an incentive function, or by considering it to be a kind of 'punishment' productive of a 'negative effect'?

Prior to any purely theoretical discussion, however, such facts as are available concerning the actual effect of the sound upon the *Ss* had best be presented. These facts are as follows:

(1) *Objective.*—In stylus maze learning without signal for error—which was the condition under which the control group worked—*S* very often withdraws finally from a cul-de-sac only after considerable delay, hesitation, fumblings, and repeated contacts with the end of the blind alley. But when such contacts produce a warning sound, *S*'*s* exploratory activity is apt to be greatly reduced and his retreat to be accomplished with much greater speed and precision. (The fact that the signal group averaged nearly 100 fewer contact errors than the control did is in part due to their less often

making the repeated contacts mentioned above.) The sound also often seemed to be more or less disturbing, even disorganizing, in that it frequently caused *S* to jump, start, or give other evidence of emotional response, especially during the early part of the practice when it was unfamiliar, or later when it came unexpectedly.

(2) *Introspective.*—The following questions were asked of all *Ss* after they had completed the learning. (*a*) "Was the sound disturbing or annoying to you?" Five males and seven females replied in the affirmative; of these six qualified their answer by such phrases as 'at first,' 'a little,' 'at times.' (The number of *Ss* who at times visibly jumped when the sound occurred greatly exceeded, however, the number who reported it to have been disturbing). (*b*) "Did you try to avoid making the sound?" All fifty *Ss* answered, 'yes.' (*c*) "Did you try to avoid it because of its characteristics as a sound or because it indicated that you had made an error?" Three males and four females gave the former reason also, though two qualified their reply by 'at first' and 'somewhat.' Generally speaking the introspective reports indicate that the sound was avoided by all *Ss* and was in itself annoying or disturbing to some of them.

To return now to the question of how the facilitating effect of the sound upon the learning is to be explained:

(1) Was the sound a 'guide'? The only possible answer is, yes. It had an informative value. It literally *announced* to *S* just where the blind alleys were, whereas in its absence much exploration was often necessary to assure him that he really was in a cul-de-sac. Hence it made the discrimination of the true path much more easy. So also, as Trowbridge and Cason [6] have pointed out, when *Ss* are trying to draw lines of various prescribed lengths the judgments 'right' and 'wrong' pronounced by the experimenter are clearly informative in their effect. Or, as Tolman [7] has argued, when *Ss* are learning which one of each of thirty pairs of holes in a punchboard maze is the correct one to choose, the use of a buzzer or an electric

[6] M. Trowbridge and H. Cason, An experimental study of Thorndike's theory of learning, *J. Gener. Psychol.*, 1932, 7, 245–260.

[7] Tolman *et al.*, *op. cit.*

shock to indicate either a correct or an incorrect choice has an 'emphasizing,' a 'signal' value.

(2) Was the sound in any sense a "punishment"? Again the answer must be, yes; and this conclusion holds good whether we define a punishment as a stimulus productive of a negative response, or as one arousing an introspectively reported annoyance or discomfort. For it is evident that the sound had both of these consequences for our *Ss*; and whether it possessed them by reason of its intrinsic characteristics *qua* sound or because of its relationship to the motive of learning the whole maze as soon as possible is not in this particular connection a matter of vital importance. Furthermore the sound did have a 'negative effect,' provided, that is, that this effect be identified with either of the two consequences of punishment cited above. This conclusion that signals for error do have a certain punishment function is in disagreement with Tolman [8] who, in his punchboard maze experiment above referred to, writes: "But bell alone seems hardly a matter of reward or punishment. Rather it seems to be a signal or emphasizer. . . . The true rewards and punishments—satisfiers and annoyers—are the ultimate 'getting-to' or 'not-getting-to' the end of the task and the getting there successfully. The effect of the bell (or of the word 'right' or 'wrong') is merely to emphasize." And yet to attribute a guidance function to such signals surely does not require that all reward-punishment significance be therefore denied them.

(3) Was the sound an incentive? It may have been to some degree for all *Ss*; it certainly had this function for a few. According to Leuba [9] any stimulus is an incentive in so far as it arouses or augments the incentive attitude, which he terms a 'source of energy' leading to movements which are faster, more vigorous, more persistent, and more uniformly relevant to the task at hand. In the present instance the sound did tend, as we have seen, to bring about quicker retreats from blind alleys, and also did of course greatly improve the accuracy of response. It is quite possible that it increased the

[8] Tolman *et al.*, *op. cit.*, p. 611.

[9] C. J. Leuba, A preliminary analysis of the nature and effects of incentives, *Psychol. Rev.*, 1930, **37**, 429–440.

general relevance of *S's* reactions by aiding the maintenance of the set, attitude, direction of attention most favorable to the learning of the maze. As to whether the sound also increased the vigor and persistence of response, one might suggest that very possibly any stimulus which functions over a period as something to be avoided comes to possess some such influence. For one thing such stimuli are apt to arouse a slight emotional response in many *Ss* and a quite marked one in a few; and such emotional reactions may have a very considerable sthenic effect. Finally the fact that seven *Ss* testified that they had tried to avoid making the sound partly because of its intrinsic characteristics should be recalled. In this connection we again disagree with Tolman. He asserts, as we have noted, that the true effect of his auditory signal was only to emphasize, since the true rewards and punishments (*i.e.* incentives) were success or failure in the task as a whole. Had he also questioned his *Ss* as to their attitude towards the sound, to say nothing of the electric shock, he might also have found some who would have reported an avoidance not determined solely by the relation of those stimuli to the learning of the maze in its entirety.

In answer, then, to the original question as to how the facilitating effect of the signal for error upon learning is to be explained, we would conclude that this effect was due to the sound's possessing not one but three functions; namely, those of a guide, of a punishment, and of an incentive. This conclusion does not, however, require the presumption that it had all of these functions at all times for every *S*; its incentive aspect in particular can perhaps not be so universally assumed. Furthermore no evidence is available on the basis of which the relative contribution of these various functions to the acceleration of the learning can be ascertained.

It is, we think, worth suggesting at this point that psychologists sometimes err in the direction of over-simplification when they treat a stimulus to which many varied functions could be attributed as if, in their experiments at least, it had exerted only one type of influence. Tolman's limiting the significance of his sound and shock signals to an emphasizing,

guiding one seems to us to be a case in point. Thorndike [10] also, in his recent experimental and theoretical contributions, has consistently regarded the words 'right' and 'wrong' as rewards and punishments influencing selection only through their satisfying and annoying effects, and has failed to suggest any other function for them. There is of course no reason to deny that these verbal stimuli do act as rewards and punishments; but there is also no reason why absorption in this aspect of their influence should wholly exclude any recognition of the fact that they may be looked upon as being both guides and incentives as well.

Thorndike [11] has also committed himself to the view that satisfiers are far more 'universal, inevitable, and direct' in their effect on learning than annoyers are, this conclusion devolving mainly from the fact that in his experiments the word 'right' produced the selection of the correct responses whereas the word 'wrong' did little or nothing to eliminate the incorrect ones. And while he admits that annoying consequences may under certain circumstances have considerable educative value, he does in general minimize their influence very greatly. Furthermore, since his verbal rewards and punishments may also be regarded as guides, or even as incentives, his conclusions if accepted would likewise tend to raise serious doubts as to the value for learning of all information of error on the one hand and the whole range of negative incentives on the other. In the present experiment, however, it seems clear that a stimulus which, as we have seen, may be variously looked upon as a punishment, a guide to error, or a negative incentive, did facilitate learning. The denial to such stimuli of any selective value whatsoever is therefore, as Thorndike himself admits, at present quite unjustified.

(*B*) *Retention.*—Our results with respect to the retention of the maze on the part of the signal and the control groups are somewhat ambiguous, since although the former group is superior in recall, in relearning and in percent saved per trial, error and second of the learning, it is only equal in percent

[10] E. L. Thorndike, *Human Learning*, N. Y., Century, 1931, 206 p. Also *The Fundamentals of Learning*, N. Y., Teachers' College, 1932, 638 p.

[11] Thorndike, *The Fundamentals of Learning*, p. 276.

saved without such qualification; furthermore none of the above differences is statistically reliable. Nevertheless we incline to the view that the weight of existing evidence and theory favors the conclusion that the retention of groups thus aided during the learning is apt on the whole to be superior to that of unaided *Ss*. The results of studies employing guidance of the manual and mechanical types support this generalization more than they do the converse one.[12] The administration of electric shock for errors during the learning appears to be of advantage for retention, as the experiments of Bunch and McTeer [13] and Crafts and Gilbert [14] indicate; and, as we will show later, the effect of the shock and the signal upon both learning and retention seem to be very similar. Furthermore, the fact that the signal group learned the maze in less time and with fewer trials and errors than the control did should be given some weight. For even if two modes of learning yield the same retention scores, the contribution to retention of every unit of learning time, or of every learning trial or error, is greater in that mode in which the amount or number of these was least. Finally, learning was better with the signal than without it, and in general one would expect that the better learned, or that learned under the more favorable condition, would be the better retained. In the present instance it seems probable that the fewer errors, the more prompt abandonment of incorrect responses, the consequent greater precision and certainty of reaction which the signal brought about, would favor the retention of a maze thus acquired.

2. *Comparison of the Signal and Shock Groups*

(*A*) *Learning.*—In learning there are no consistent differences between the signal and shock groups, though both are reliably superior to the control. These results are in agree-

[12] See H. L. Koch, The influence of mechanical guidance upon maze learning, *Psychol. Monog.*, 1923, **32**, No. 147, 113 p. R. H. Waters, The influence of large amounts of manual guidance upon human maze learning, *J. Gener. Psychol.*, 1930, **4**, 213–228. R. H. Water and A. L. Ellis, The relative efficacy of free and guided learning when equated in terms of time, J. Compar. Psychol., 1931, **12**, 263–277.

[13] M. E. Bunch and F. D. McTeer, The influence of punishment during learning upon retroactive inhibition, this JOURNAL, 1932, **15**, 473–495.

[14] L. W. Crafts and R. W. Gilbert, *op. cit.*

ment with those of Tolman [15] and Lorge [16] in so far, that is, as their investigations are comparable with the present study. In Tolman's experiment already referred to, having a buzzer sound when *S* made a wrong choice, *i.e.* punched with a stylus the wrong one of a pair of holes in a punchboard maze, was if anything superior to sounding a buzzer and at the same time giving an electric shock. In Lorge's experiment *S* had to select one of four words, nonsense syllables or numbers, and if wrong was 'punished' by the word 'wrong' and a shock. Lorge found that even the addition of the shock did not give the punishment any more than the zero value for selection which Thorndike had previously obtained for the word 'wrong' used alone. Hence we have two experiments in which a signal (or punishment) for error, auditory or verbal, was quite as effective for learning as that same signal plus an electric shock; in the present experiment a sound and a shock, each used alone and separately, were also of equal value.

In explanation of these results the following may be suggested: The guidance or informational value of a sound, or of the word 'wrong,' for error is just as great as that of a shock. Hence from this viewpoint nothing could be gained by the addition or substitution of the latter. If a difference in favor of the shock had been found, it could have been attributed only to its presumably greater incentive and punishment functions. But while one might suppose that electric shock would be a strongly motivating stimulus for most *Ss*, it might also be for many a disrupting and disorganizing one. If, in the group averages, one of these two effects were counterbalanced by the other, the net result of the shock would be a facilitation due only to its guidance value; and this, as we have seen, was possessed equally by the auditory signal.

(*B*) *Retention.*—In retention the signal group is slightly superior to the shock group in recall; otherwise there are no consistent differences between them. The absence of any other experiments in which retention has been studied under these two conditions leaves us with no data with which these

[15] Tolman, *op. cit.*

[16] Lorge, *op. cit.*

results may be compared. In view, however, of the very similar effects of the signal and the shock upon learning, this failure to find any difference in retention is of course not surprising.

Summary

The aims of the experiment were to determine the effect upon the learning and the retention of a stylus maze of giving an auditory signal-specifically a buzzer sound and a click—for error during the learning, and also to compare these results with those of a previous study in which an electric shock had been similarly administered. The *Ss* were one hundred college students divided into two groups of twenty-five men and twenty-five women each. Both groups learned the McGeoch-Melton medium maze to the criterion of two of three successive trials without error. In the experimental group every contact of the stylus with the end of a cul-de-sac produced the above sound. After an interval of one week the maze was relearned to the same criterion as before, but without sound for either group. The results were: In learning the experimental group was markedly superior to the control according to the usual three criteria of trials, errors and time. In retention it was equal in per cent saved, but superior in recall, in relearning, and in per cent saver per trial, error and second of the original learning. Its superiority in learning was attributed to the unquestionable guidance or informative value of the auditory signal, together with the punishment and incentive functions which it probably also possessed. Comparison of the signal and the shock groups showed the value of sound and shock for both learning and retention to have been approximately equal. The guidance value of these two stimuli may be considered identical, and perhaps the disturbing effect of the shock on some *Ss* counterbalanced its presumably greater significance as an incentive and as a punishment.

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THE COMPARATIVE EFFICIENCY OF VARIED AND CONSTANT METHODS IN SENSORIMOTOR LEARNING

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The investigation described herein was an attempt to determine objectively whether the employment of a constant method is more effective than varied methods in the learning process. The experimental practice was divided into two distinct phases. In the first phase practice was undirected for both groups. Each subject employing a constant procedure chose his own method and adhered to it throughout the periods of practice. Subjects who employed varied methods were required to select four different procedures which were adhered to alternately during practice.

In the second phase subjects of the undirected group were allowed to choose their own method or methods of practice, while the subjects of the other group were directed in using the most effective method.

Types of Activities Employed

The activities engaged in in this study were ball tossing, quoit pitching and 'gowf.' It may not be necessary to describe in detail the activities of ball tossing and quoit pitching, but 'gowf' is probably not so well known. 'Gowf' is a flipping game that is played in a shallow, open card-board box which is approximately eight inches long, six inches wide and one inch deep. The box may be held in one or both hands while a steel ball about three eights of an inch in diameter is flipped from six different positions into six corresponding shallow wooden cups which are glued to the bottom of the box. These cups are about three fourths of an inch in diameter and one fourth of an inch deep. The objective in this game is to flip the ball into the six cups from the six different positions with as few trials as possible, six successful trials making a perfect score. Subjects were required to practice flipping for 18 rounds. All 18 rounds were made in one period of an hour, more or less, the time required depending upon the ability of the subject and method used. In the 18 rounds the ball had to be successfully flipped into 108 cups. Subjects using a varied procedure were required to choose at least four different methods during an orientation period of five minutes. They then used these four methods alternately, a different method for each round, until they

had completed the 18 rounds. Subjects using a constant method selected, during the orientation period, what seemed to them to be the most effective method, and each subject proceeded to use the method he chose for the 18 rounds.

The apparatus for practice in ball tossing consisted of five golf balls and a closed wooden box rectangular in shape, twelve inches high, six inches wide and six inches deep. At the top of one side of the box a four inch square hole was cut through which the balls were tossed into the box. The box was suspended from a hook so that it hung on the wall with the bottom of the four inch opening just five feet from the floor. Parallel with the front face of the box a line was drawn on the floor six feet from the wall, and from behind this line the subject did his pitching. Each day 100 pitches were required over a period of 18 days, making, 1,800 pitches in all. The pitches were made in groups of ten tosses.

By the varied procedure it was stipulated that the subject should have at least four different methods of tossing, and that after tossing ten balls by one method he should change to another method. The students chose their own methods which consisted in variations in pitching by changing standing position, way of holding ball while it was being tossed, a shift in tossing from right to left hand, under hand tossing, and straight or over hand tossing, and the like.

With the constant learning procedure it was stipulated that one method was to be used. Five minutes were allowed, also, for the subjects of this group to orient themselves in the task and for the selection of the method to be followed throughout the practice period.

In quoit pitching the apparatus consisted of ten heavy rubber rings, and a nine inch square block one and a half inches thick. In the center of the block was secured an upright post a half inch in diameter and six inches high. The rubber rings were approximately five inches in outer diameter and three inches in inner diameter. The subject stood behind a line which was six feet from the nearest edge of the stand and endeavored to 'ring' on the upright post as many quoits as possible out of 100 trials each day. The same rules and stipulations governed the quoit pitching with respect to selection and change of method as have been described for ball pitching.

Selection of Groups

In the first phase of the comparison one hundred and sixty-one college students were employed as subjects, fifty-two of whom were men and one hundred and nine were women. These subjects were regular students of the general and educational psychology classes of the Mississippi Delta State Teachers College during the session of 1932–1933. The groups were arbitrarily assigned by classes in the procedure that was employed. For the three different activities two groups of general psychology students who employed a constant procedure were compared to two other groups in the same subject who used varied procedures. Likewise, one group in educational psychology was compared to another in educational psychology. The subjects who used a constant method in ball tossing and quoit pitching numbered 73 while the subjects employing the varied procedure numbered 88. These groups were alternated in 'gowf' so that the constant group numbered 88 and the varied group numbered 73. The group which employed a constant procedure in one form of activity thus became the varied group in another, and this alternation, perhaps, had a tendency to equalize some factors which were not considered, or which could not have been weighted had they been known.

A distributed practice schedule was arranged in which different periods throughout

the morning and the afternoon were used. The subjects, employing either constant or varied procedures, were so distributed that in morning or afternoon practice the numbers employing methods to be compared were approximately equal. If there is a most favorable time of day for practice in activities like those mentioned above, such a selective factor may have been taken care of by the distributed practice schedule of these different sections.

Summary of Undirected Phase of Learning

In an inspection of the summarized data, as revealed by Table I, it is seen that in ball tossing the 73 students who em-

TABLE I

Summary of All Groups in Ball Pitching

Constant Groups		Varied Groups	
N	Totals Pitched	N	Totals Pitched
31	18,272	33	19,025
20	12,365	35	16,315
22	13,372	20	10,580
Totals 73	44,039	88	45,920
Means	603.7		521.8

Obt. M's dif. 85.5
S.E. dif. 26.8

TABLE II

Summary of All Groups in Quoit Pitching

Constant Groups		Varied Groups	
N	Totals Pitched	N	Totals Pitched
31	17,906	33	14,047
20	9,506	35	11,910
22	9,907	20	7,912
Totals 73	37,319		33,869
Means	511.2		384.9

Obt. M's dif. 122.2
S.E. dif. 25.7

ployed a constant method had a significant advantage over the 88 students who employed a varied procedure. The reliability of this advantage is indicated by a critical ratio of 3.2 which is shown in Table IV. Likewise, in quoit pitching, and

TABLE III

SUMMARY OF ALL GROUPS IN "GOWF" GAME[1]

Constant Groups		Varied Groups	
N	Totals Pitched	N	Totals Pitched
33	14,522	31	21,137
35	18,697	20	15,185
20	8,164	22	17,638
Totals 88	41,383	73	53,960
Means	470.3		739.2

Obt. M's dif. 263.4
S.E. dif. 43.7

TABLE IV

RELIABILITY OF DATA FOR ALL GROUPS THROWN TOGETHER

Materials	Method	N	Obt. Means	S.D.	S.E.	Diff. of M's	S.E. Diff.	Critical Ratio
Balls	Constant	73	601.0	169.0	19.9	85.5	26.8	3.2
	Varied	88	515.5	172.5	18.0			
Quoits	Constant	73	508.0	186.5	21.9	122.2	25.7	4.8
	Varied	88	385.8	131.0	13.9			
Gowf[1]	Constant	88	474.0	194.0	20.7	263.4	43.7	6.0
	Varied	73	737.4	327.0	38.4			

[1] A smaller number of pitches indicates greater skill.

in 'gowf,' the critical ratios of 4.8 and 6.0 indicate a significant advantage for a constant method over varied methods in learning. Such critical ratios as the above permit a conclusion that the chances would be better than 99 in 100 that the true difference between learning by constant methods, in activities such as have been described, and learning by varied methods will likely be greater than zero.[1]

These differences may be expressed another way, that is, in terms of percent based on the differences between the means as shown in Table IV. The advantage for the constant group was 17 percent in ball tossing, 32 percent in quoit pitching and 35 percent in 'gowf.'

Students who selected constant methods in this phase of

[1] Garrett, H. E., Statistics in Psychology and Education, 132–133.

the investigation were undirected in their selection, and doubtless in many instances chose ineffective procedures. On the other hand, students who used four different methods often employed one effective method in practice. The outcome of undirected selection of methods had a tendency, perhaps, to bring the two groups together due to the fact that the varied methods group had a choice of four procedures and often approximated the most effective procedure which could be used.

Undirected and Directed Learning

From a group of fifty students who were taking general psychology during the fall quarter of the session of 1933–1934, forty subjects were selected with a view of comparing practice with a directed effective method to practice with an undirected and in part a varied procedure. By means of a preliminary test these forty subjects were so paired in two groups that in 'gowf,' the activity employed in the last phase, the two groups were approximately equal. In this preliminary test all subjects flipped the steel ball for ten minutes under similar conditions with the exception that they were allowed to flip by any method or methods they chose. They were encouraged to pitch the ball into as many cups as possible during the ten minutes. Table V gives the total number of pitches made by each student in the ten minutes. It also gives the average number of pitches required to make each cup. The two groups, A and B, were selected on the basis of the total number of pitches made, and the average number of pitches required per cup.

Results of End Tests

In the end test Group A practiced first according to the method or methods used in the preliminary test. Practice for 18 rounds in one period, or the successful flipping of the ball into 108 cups, was required as in the first phase of the experiment. After Group A had completed the end test Group B was shown the most effective method of flipping and was required to adhere to this one method for 18 rounds. The time required for flipping was kept for each subject of both groups.

TABLE V

PRELIMINARY TEST

Group A[1]			Group B[1]		
Subject	Cup. Avg.	Pitches in 10 min.	Subject	Cup. Avg.	Pitches in 10 min.
1	1.9	125	1	1.7	208
2	2.1	178	2	1.8	165
3	2.1	203	3	2.8	204
4	3.2	259	4	3.3	208
5	3.6	139	5	3.4	122
6	4.2	169	6	4.5	128
7	4.1	147	7	4.7	139
8	4.4	106	8	4.7	236
9	5.5	162	9	4.8	173
10	5.6	116	10	5.0	159
11	5.6	205	11	5.1	203
12	5.9	155	12	5.4	223
13	6.2	244	13	6.0	181
14	6.4	199	14	6.5	124
15	6.5	176	15	6.7	202
16	6.5	150	16	7.8	164
17	8.3	200	17	8.5	248
18	8.4	134	18	8.8	115
19	9.6	144	19	9.6	212
20	9.8	148	20	9.7	145
Totals	109.9	3359	Totals	110.8	3559
Means	5.5−	167.9	Means	5.5+	177.9

[1] Each group had 14 women and six men as subjects.

It was noted in the end test that ten subjects of Group A selected approximately the most effective method as was given to all subjects of Group B. The ten remaining students of Group A obviously employed one or more ineffectual methods of practice. When a comparison is made it is seen in Table VI that the directed or constant group showed a reliable superiority of about 37 percent in flipping with a 25 percent saving in time over the varied undirected methods group even though half of the undirected group chose approximately the most effective method.

In Table VII data are given which show how six students of group A improved after being given the effective method for practice. These students voluntarily expressed a desire to compare their ineffective methods with the effective method. With the effective method they made a gain over their first

TABLE VI
END TEST

Group A (Undirected)				Group B (Directed)			
No.	18 Rounds Cup Avg.	Total Pitches	Minutes	No.	18 Rounds Cup Avg.	Total Pitches	Minutes
1	1.6	176	12	1	1.6	171	7
2	1.9	205	10	2	1.4	149	8
3	1.9	206	9	3	2.6	232	12
4	2.7	293	13	4	3.3	354	17
5	3.6	394	25	5	2.8	305	14
6	2.7	287	20	6	1.7	184	15
7	3.4	367	20	7	1.9	190	15
8	3.1	335	15	8	2.1	230	11
9	4.5	491	20	9	3.1	334	14
10	4.5	491	35	10	2.4	259	18
11	5.4	584	25	11	3.2	349	21
12	8.1	952	45	12	2.5	272	15
13	2.9	315	20	13	2.3	256	17
14	2.8	314	18	14	2.6	288	16
15	3.7	399	16	15	2.6	287	13
16	5.3	537	23	16	4.5	495	40
17	5.6	604	42	17	2.8	309	12
18	5.8	638	25	18	3.6	387	32
19	9.2	991	25	19	4.8	485	15
20	4.8	521	30	20	3.7	388	20
Totals	81.6	9,100	448		55.5	5,924	332
Means	4.2	455.0	22.4		2.8	296.2	16.6
Obt. M_1	450.0			Obt. M_2	287.9		
S.D.	2.5			S.D.	2.8		
S.E.	.56			S.E.	.63		

$M_1 - M_2$ = 167.1
S.E. dif. = .9
Crit. Rat. = 185.7

TABLE VII
COMPARISON OF PRACTICE RESULTS OF SIX STUDENTS OF GROUP A WHO USED THE DIRECTED METHOD AFTER USING VARIED METHODS IN THE END TEST

Subject	Varied or Undirected Methods		Constant or Directed Method	
	18 Rounds Total	Cup Average	18 Rounds Total	Cup Average
9	491	4.5	361	2.1
11	584	5.4	391	3.4
12	952	8.1	377	3.3
16	537	5.3	255	2.3
18	638	5.8	296	2.5
20	521	4.8	246	2.2
Totals	3,723	33.9	1,926	15.8
Means	620.5	5.3	321	2.6

practice of about 50 percent. It may be argued that previous practice for one period helped to raise the score of these six students in their second trial with an effective method. On the other hand, habits formed in ineffectual practice could have had just the opposite effect in hampering improvement when the effective method was employed.

General Summary

The purpose of this study was to compare the effectiveness of constant and varied methods in sensorimotor learning. Two hundred and one college students were employed in ball tossing, quoit pitching and 'gowf.' In the first phase of the investigation one hundred and sixty-one subjects were employed in the different activities in undirected learning. There were 88 subjects in one group and 73 in the other. One group employed varied methods for one form of sensorimotor activity while the other group used a constant procedure for the same type of activity. Later, the former group practiced another type of activity by the constant method while the latter group employed a varied procedure. The findings indicate a reliably significant superiority of about 30 percent for constant methods over varied methods in learning the types of activities herein described.

In the second phase of the investigation twenty subjects using an undirected method or varied procedures in 'gowf' were compared to an equated group employing a directed constant method. The directed constant method appeared to be significantly superior to the undirected method by more than 35 percent in flipping efficiency and 25 percent in economy of time.

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TRAINING EFFECTS IN MOTOR SKILLS

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Seashore (1930) has listed four general hypotheses to account for individual differences in complex motor performances. According to these hypotheses individual differences may be determined by: (1) a general motor function and other special abilities, (2) a number of simple variables such as speed of reaction-time, reflexes, steadiness of hand, etc., (3) a number of serial motor coördinations, called 'basic motor capacities,' or (4) differences in training, both direct and transferred, and including informal types of training.

This study is principally concerned with the fourth of these hypotheses. It attempts to determine to what extent training along certain lines has influenced motor performances in ways variously related to the particular training received. More specifically it consists of a comparison of the motor performances of a group of pianists and non-pianists in order to determine whether or not pianistic practice has brought about any superiority in speed, accuracy, and steadiness of arm movements on the part of the musically trained subjects. A study of sex differences in motor abilities is also made.

For this purpose two separate experiments were performed. The first consisted in comparing a group of 26 pianists who had had at least three years of piano training with a comparable group of 16 subjects who had not had such training.

In doing this three series of tests were given to each subject. Each subject was tested individually and the entire series of tests required about forty minutes for their completion.

The first series consisted of six tapping exercises as follows: (1) simple tapping with the second and third finger simultaneously, (2) alternation of second and third fingers, (3) alternation of third and fourth fingers, (4) alternation of fourth and fifth fingers, (5) alternation of fifth, fourth, third, and

second fingers, and (6) alternation of first, fourth, and fifth fingers. Each exercise was continued for five seconds and the score was the number of taps made in the period. The subject tapped a telegraph key and records were made on a recording kymograph. Each subject was told and shown just what to do and a short preliminary practice period of one or two seconds was permitted before beginning each exercise. The subjects were instructed to tap as rapidly as possible. Records were obtained from both the right and left hands.

The results of these exercises are given in Table I.

TABLE I

COMPARISON OF PIANISTS AND NON-PIANISTS IN RATE OF TAPPING FOR SIX DIFFERENT EXERCISES

(*a*) *Right Hand*

	Musicians		Non-Musicians		Difference Between Means	Probable Error of Difference	Critical Ratio
	Mean	Standard Deviation	Mean	Standard Deviation			
1	30.04	2.72	28.75	2.68	1.29	.18	7.17
2	41.80	2.19	33.63	4.15	8.17	.76	10.75
3	32.21	2.85	28.35	3.37	3.86	.68	5.68
4	24.82	4.24	21.50	3.28	3.32	.78	4.26
5	27.83	2.83	20.94	4.87	6.89	.90	7.66
6	25.35	4.54	18.75	4.22	6.60	.92	7.17

(*b*) *Left Hand*

	Musicians		Non-Musicians		Difference Between Means	Probable Error of Difference	Critical Ratio
	Mean	Standard Deviation	Mean	Standard Deviation			
1	27.95	2.77	25.75	3.15	2.20	.20	11.00
2	38.04	4.88	25.89	4.39	12.15	.98	12.40
3	32.83	3.38	22.31	2.36	10.52	.19	55.37
4	27.61	2.41	21.75	3.34	5.86	.20	29.30
5	24.26	2.94	21.50	4.07	2.76	.76	3.49
6	23.35	4.59	16.38	4.92	6.97	1.02	6.83

This table indicates a significant superiority of the performances of the musicians as compared with the non-musicians. The average difference between the means of the two groups on all of the exercises for the right hand is 5.02 and for the left hand 6.72. It will be noted that the difference between the pianist and non-pianist is greater for the left than

for the right hand. This indicates that pianistic training has increased the speed of the non-preferred more than it has the preferred hand. This is in keeping with results on other processes which show greatest practice effects in the least practiced functions.

When the exercises were selected an attempt was made to arrange the series in order of increasing difficulty. The results indicate that we were not entirely successful in this attempt. Exercise 2 (alteration of second and third fingers) seems to be somewhat more rapid than simple tapping with the same fingers simultaneously. Next in order of difficulty (on the basis of averages) are 3, 4, and 5 (all approximately equal) and six which is definitely more difficult as evidenced by speed of reaction.

It is interesting to observe in which exercises the musicians show the greatest superiority over the non-musicians. The writers had assumed a priori that the greatest differences would be found with the most difficult exercises. Averaging the differences for the right and left hands we find the greatest difference in 2 (alternation of second and third fingers) next in order follow 3, 6, 5, 4, and 1. Thus there seems to be no relationship between difficulty of exercise and amount of difference between the two groups.

The second test given the subjects was a modification of the Whipple "Test of Involuntary Movement" (see Whipple, 1914). This test was devised for measuring the amount of movement taking place when the fingers, hand, and arm are held as motionless as possible.

In order to obviate some of the difficulties involved in the ordinary procedures and scoring methods commonly used, two different procedures were followed. In the first, the subject placed the stylus within each of the holes piercing the metal plate in succession and held it there for fifteen seconds without it touching the sides. The diameters of the holes were 32, 20, 13, 11, 10, 9, 8, and 7 sixty-fourths of an inch respectively. The subject started with the largest hole and proceeded with each smaller one until he found the smallest one in which he could hold the stylus 15 seconds without making contact with

the plate. A telegraph sounder in circuit with the plate and stylus sounded each time the pointer came into contact with the sides. Both left and right hands were tested. The score was taken as the number of the smallest hole done satisfactorily when the largest hole was numbered 1 and the smallest 8.

The second variation of this test was to have each subject place the stylus within each hole to a depth of 1/2 inch and immediately withdraw it without coming into contact with the sides. This was more a test of accuracy of a single continuous voluntary movement than was the preceding exercise.

The score was again the number of the smallest hole in which no contact was made. Both hands were tested as before.

The results of these two exercises are given in Table II.

TABLE II

COMPARISONS OF STEADINESS RECORDS OF PIANISTS AND NON-PIANISTS UNDER TWO DIFFERENT CONDITIONS

	Musicians		Non-Musicians		Difference Between Means	Probable Error of Difference	Critical Ratio
	Mean	Standard Deviation	Mean	Standard Deviation			
First Time							
Right hand.....	4.41	1.62	3.94	1.43	.47	.101	4.7
Left hand.....	3.44	1.52	3.25	1.50	.19	.101	1.9
Second Time							
Right Hand.....	5.30	1.80	4.75	1.58	.55	.111	5.0
Left hand.....	4.00	1.68	3.44	1.32	.56	.099	5.6

In the table the first measures are for the exercise in which a 15-seconds interval was used and the third and fourth lines give the results for the second exercise.

Here again, the musicians show a marked superiority over the non-musicians. The differences are greater for the second exercise than for the first. This is probably due to the fact that piano playing requires accuracy of continuous voluntary action more than it does steadiness in maintaining a given stationary position of the arm and hand.

The third series of tests made use of the 'Bagley Tracing Board.' These tests consisted in requiring the subject to pass

a stylus along a narrow slit bounded by two metallic strips and recording how far the subject could go before touching the sides. The slit gradually narrowed toward the end. Five trials were taken with each hand under each of two conditions. First five trials with each hand were taken with the apex of the tracing board pointing toward the subject, the movement being toward the body. Another series of five trials with each hand was taken when the board was placed parallel with the edge of the table nearest the subject. The speed of movements was controlled so that the subjects would traverse the full length of the board in approximately nine seconds. A telegraph sounder in circuit with the board and stylus sounded each time contact was made with the sides of the pathway. The subject was stopped when the side was touched and the point on the scale where the contact occurred was read. In all cases the body was motionless, the movement being made entirely by the unsupported arm. The mean of the five readings was taken as the subjects score for each hand under each condition and two series of measurements were made.

Tables III and IV give the results of these measurements.

TABLE III

SCORES MADE BY PIANISTS AND NON-PIANISTS IN A STEADINESS TEST WHEN THE APEX OF THE TRACING BOARD WAS POINTING TOWARD THE SUBJECT

	Pianists		Non-Pianists		Difference Between Means	Probable Error of Difference	Critical Ratio
	Mean	Standard Deviation	Mean	Standard Deviation			
First Trial							
Right hand.....	20.73	3.86	17.85	5.07	2.88	.176	16.00
Left hand.....	17.96	4.48	13.55	4.69	4.41	.917	4.79
Second Trial							
Right hand.....	20.26	3.07	16.10	1.69	4.16	.150	27.73
Left hand.....	17.56	3.43	12.90	4.39	4.66	.796	5.83

Again the pianists show a decided superiority over the non-pianists. As was true for speed of reaction, the differences are again greater for the left than for the right hands.

TABLE IV

SCORES MADE BY PIANISTS AND NON-PIANISTS WHEN THE TRACING BOARD WAS PLACED PARALLELING THE TABLE AND THE STYLUS WAS DRAWN FROM LEFT TO RIGHT

	Musicians		Non-Musicians		Difference Between Means	Probable Error of Difference	Critical Ratio
	Mean	Standard Deviation	Mean	Standard Deviation			
First Trial							
Right hand.....	20.85	3.66	19.25	4.31	1.60	.809	1.98
Left hand.....	15.45	4.65	12.50	.61	2.95	.193	15.53
Second Trial							
Right hand.....	20.76	3.07	18.45	3.78	1.81	.694	2.62
Left hand.....	15.04	3.88	13.55	3.37	1.49	.714	2.10

SEX DIFFERENCES IN MOTOR SKILLS

In the previous experiment the two sexes were equally represented in the two groups and no attention was paid to sex differences. Some of the same measurements as were made in the previous section were taken on a group of 68 males and 75 females and the two groups compared in order to determine the extent of sex differences. All of these subjects were members of the senior author's class in general psychology at the University of North Dakota. The measurements taken were rate of simple tapping, steadiness as measured by the Whipple apparatus (15 seconds test period), and scores on the tracing board (board parallel to table). Instead of dealing with the right and left hands separately, the average of the records for the two hands was taken as the person's score on each test. Consequently each person's record on each test is represented by a single measure.

The results of the comparisons of these two groups are given in Table V.

TABLE V

SEX DIFFERENCES BETWEEN 68 MALES AND 75 FEMALES IN THREE DIFFERENT MOTOR PROCESSES

	Speed of Tapping				Steadiness Tester				Tracing Board			
	Mdn.	Q.	Diff.	C.R.	Mdn.	Q.	Diff.	C.R.	Mdn.	Q.	Diff.	C.R.
Male.....	39.55	2.54	4.80	7.89	5.25	.85	.44	3.14	23.31	1.51	3.75	8.93
Female...	34.75	3.38			4.81	.84			19.56	2.45		

This table shows a statistically reliable superiority of the males over the females in all three series of tests. The difference is much greater relatively for speed of tapping and scores on the tracing board than for the Whipple steadiness tester.

Summary and Conclusions

In this study we have found a consistent motor superiority of a group of pianists over a comparable group without pianistic training. We have also found a sex difference in favor of males. These differences show themselves in speed, accuracy, and steadiness of arm and finger movements.

We can do little more than speculate on the probable causes of these differences. The most plausible explanation of these observed differences is on the basis of practice effects both direct and transferred. The pianistic training seems to have developed a motor ability which effects speed, accuracy, and steadiness of arm movements. Whether practice effects can explain the observed sex difference is debatable. The correlations between these different exercises do not seem sufficiently high to indicate the existence of a general motor function. The correlation between speed of tapping and scores on the tracing board is .13 $\pm$ .06, between tapping and steadiness of movement .43 $\pm$ .05, and between steadiness and score on the tracing board .24 $\pm$ .06. These results agree with those of Muscio (1922), Seashore (1930), and others who find rather low positive correlation between various measures of motor capacity.

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A PRELIMINARY INVESTIGATION OF THE EFFECTS OF MENTAL DISTRACTION UPON MUSCULAR FATIGUE

BY DAVID G. RYANS

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The amount of fatigue *felt* by an individual seems, to an extent, dependent upon attitude toward the task involving expenditure of effort, and it seems to vary from time to time for the same function. This problem appears to be analogous to that of empty and filled time. When we are occupied time seems to pass more quickly. Likewise, one sometimes senses little or no feeling of tiredness from an undertaking that would, under other circumstances, literally wear him out. Feats necessitating almost incredible strength and endurance are frequently performed during moments of excitement.

In this investigation an attempt was made to determine if and to what extent mental set or attitude affected muscular fatigue resulting from ergograph operation. We hoped to discover, if possible, whether fatigue was actually alleviated when the individual was engaged with mental work, or if the local sensations were simply blocked off by such activity; whether the organic state or only the *feeling* was concerned.

The ergograph records were obtained upon the smoked drum of a clock-driven kymograph. The subject was seated in a comfortable position and so that his right arm and hand lay flat, with palm up, on the arm-rest of the apparatus. A stirrup, attached to an eight pound (3.628 kgms.) free hanging weight, was taped to the joint nearest the nail of the second finger. The weight was alternately drawn and released by the finger to the beat of a metronome set at 140 beats per minute (70 *complete* strokes per minute.) Each subject participated in a series of eight one-minute trials interspersed with two-minute rest periods between trials. For trials 1, 3, 6, and 8 the subject was instructed to do his best and to keep his attention directed upon his finger movements. For trials, 2, 4, 5, and 7 instructions were to read aloud successive paragraphs of E. A. Poe's *The Tell-tale Heart* while operating the ergograph. The subjects were eight right-handed men students who had been previously trained to lift an ergograph weight to the beat of the metronome.

The heights of every tenth stroke, and of the three strokes directly preceding and the three strokes directly following the tenth stroke, on the kymograph records were

measured in centimeters. The median of these seven measurements was accepted as an indication of the relative ability to lift the weight at the given time. Thus, seven readings (at the 10th, 20th, 30th, 40th, 50th, 60th, and 70th strokes) were obtained for each one-minute trial.

Following this procedure it was possible to make certain comparisons of work decrement under the varying conditions (reading and non-reading) of the experiment.

The results are presented in Tables I, II, III, and IV.

Table I shows the means and standard deviations per

TABLE I

MEANS AND STANDARD DEVIATIONS, PER READING AND PER TRIAL, OBTAINED FROM THE ERGOGRAPH RECORDS OF EIGHT SUBJECTS *

Stroke	Trial															
	1		2		3		4		5		6		7		8	
	Mn.	S.D.	Mn.	S.D.	Mn.	S.D.	Mn.	S.D.	Mn.	S.D.	Mn.	S.D.	Mn.	S.D.	Mn.	S.D.
10	*3.90*	0.77	*3.50*	0.68	*3.46*	0.36	*2.82*	0.48	*2.75*	0.24	*2.86*	0.38	*2.40*	0.71	*2.70*	0.67
20	*3.55*	0.63	*3.31*	0.84	*3.01*	0.56	*2.30*	0.69	*2.37*	0.35	*2.71*	0.33	*2.17*	0.56	*2.39*	0.31
30	*3.60*	0.80	*3.08*	0.86	*2.40*	0.56	*2.15*	0.36	*1.80*	0.39	*1.96*	0.48	*1.71*	0.37	*1.89*	0.37
40	*3.39*	0.80	*2.35*	0.77	*1.81*	0.41	*1.76*	0.41	*1.29*	0.35	*1.46*	0.41	*1.14*	0.29	*1.16*	0.21
50	*2.90*	0.84	*1.75*	0.91	*1.35*	0.36	*1.34*	0.47	*0.96*	0.33	*1.09*	0.32	*0.76*	0.30	*0.84*	0.22
60	*2.39*	0.94	*1.21*	0.73	*1.07*	0.35	*1.05*	0.49	*0.79*	0.31	*0.87*	0.31	*0.63*	0.29	*0.71*	0.26
70	*1.79*	1.05	*0.81*	0.69	*0.89*	0.39	*0.81*	0.36	*0.70*	0.25	*0.66*	0.29	*0.58*	0.28	*0.56*	0.34
Mean	*3.07*		*2.29*		*2.00*		*1.75*		*1.52*		*1.66*		*1.34*		*1.43*	
S.D.		0.79		0.74		0.41		0.45		0.30		0.34		0.40		0.35

* Height of stroke is given in cms.

Trials 2, 4, 5, and 7 involved reading distraction, while Trials 1, 3, 6, and 8 served as the control series.

reading and per trial for the group of eight subjects.

In Table II the work decrement, per trial, is given in terms of percent loss. The reading at the 70th stroke was compared with that at the 10th stroke. With the outstanding exception occurring in Trial 1, the percentage loss is remarkably consistent from trial to trial and indicative of slightly less decrement when the reading distraction was employed. In view

TABLE II

WORK DECREMENT PER TRIAL REPRESENTED BY PER CENT LOSS WHEN THE READING AT THE 70TH STROKE IS COMPARED WITH THAT AT THE 10TH

Trial	Series	Per Cent Decrement
1	Control	54.1
2	* Experimental	76.8
3	Control	74.3
4	* Experimental	71.3
5	* Experimental	74.6
6	Control	77.0
7	* Experimental	75.9
8	Control	79.3

* Reading aloud from E. A. Poe's *The Tell-tale Heart* served as the distraction in the Experimental series.

of the rather closely agreeing results of the following periods the writer is inclined to regard the 54.1 percent decrement shown in Trial 1 as spurious and not significant enough to invalidate the tendency maintained by the remainder.

Table III shows the percent loss in ability from the first

TABLE III

COMPARISON OF DISTRACTION AND NON-DISTRACTION SERIES WITH REFERENCE TO PER CENT LOSS IN ABILITY FROM THE FIRST TO THE LAST TRIALS IN EACH SERIES

	Per Cent Loss	
Stroke	Control Series	*Experimental Series
10	30.9	38.4
20	32.7	34.1
30	53.2	44.5
40	65.8	51.5
50	71.1	56.6
60	70.3	48.0
70	68.7	28.4

* Reading aloud from E. A. Poe's *The Tell-tale Heart* served as the distraction in the Experimental series.

to the last of the trials without distraction as compared with the percent loss from the first to the last of the trials with reading. Comparisons are made at every tenth stroke. The decrement when measured at the 10th and 20th strokes seems to indicate slightly less fatigue during the control series, but at the subsequent readings differences favoring the experimental conditions seem to become increasingly evident.

Six of the eight individuals participating in the experiment (Table IV) showed *less* work decrement when *reading*, the average difference between the decrement in the control and

TABLE IV

COMPARISON OF THE WORK DECREMENT IN THE CONTROL AND EXPERIMENTAL SERIES BY INDIVIDUALS

Subject	Per Cent Decrement	
	Control Series	*Experimental Series
A	55.8	25.6
B	65.2	51.8
C	58.7	59.7
D	37.8	24.8
E	62.1	51.2
F	18.6	23.7
G	46.2	25.0
H	57.7	32.7

* Reading aloud from E. A. Poe's *The Tell-tale Heart* served as the distraction in the Experimental series.

experimental series being 18.95 percent. The two subjects who showed greater loss during the reading trials averaged 3.05 percent difference when decrement for reading and non-reading trials was compared.

For the entire group the work decrement from Trial 1 to Trial 8 (control series) was 53.4 percent, while from Trial 2 to Trial 7 (experimental series) it was but 42.4 percent. Thus, from the standpoint of average ability per trial, there was an eleven percent difference in work decrement, the *greater loss* occurring in the trials with which there was *no reading* distraction. The difference, while not statistically significant $\left(\frac{\text{D.}}{\text{S.D.(dif.)}} = .44\right)$, seems worthy of consideration.

It seems probable that mental activity, such as was involved in reading—the withdrawal of attention from the cumulative feeling of tiredness—does tend to reduce the organic fatigue effects, in some individuals, in so far as such effects may be indicated by work decrement in ergograph operation.

(Manuscript received November 9, 1933)

THE RELATIONSHIP OF EYE MUSCLE BALANCE TO THE SIGHTING EYE

BY BLAKE CRIDER

The purpose of this investigation was to determine whether unilateral sighting preferences (ocular dominance) were related to characteristic differences in the ocular muscle balance of the two associated eyes.

The commonly used screen and fixation tests were used for determining eye muscle characteristics which, for lack of a better name, are call insufficiencies.

In the fixation test the subject was required to fixate a small black disc placed at reading distance and on a level with the eyes. The disc was advanced slowly to the near point of convergence or within its immediate vicinity. The eye which diverged was considered the insufficient eye.

In the screen test the subject fixated an object 20 feet distant placed on a level with the eyes. Each eye was covered alternately with an opaque screen. The screen was removed quickly and the movement of the eye, if any, was noted. The test was repeated with the eyes fixated on the disc placed approximately five feet away.

The movement and position of each eye behind the screen as well as the movement on removal of the screen was noted. In the case of similar movements in the two eyes the relative speed of recovery of fixation was noted.

Insufficiencies were listed for (1) the eye which did not converge in the fixation test; (2) the eye which recovered its fixation more slowly on removal of the screen; (3) the eye which converged or diverged in contrast to the eye which maintained its equilibrum; (4) and the eye which made the greatest excursion behind the screen.

All of the above-mentioned routine was not used with all subjects but any part of it was employed under any situation in order to bring out any characteristic differences in the two eyes.

In summary, instead of specifically eliciting eye muscle anomalies such as esophoria, exophoria, and hyperphoria a study was made of the speed, direction, and extent of the movement in one eye with respect to similar movements in the opposing eye. Whenever no characteristic differences could be elicited or the movements of the two eyes were so similar that the insufficient eye could not be determined reliably no data were recorded. In other words, only clearcut cases were included.

With the above-described procedure 229 cases of eye muscle insufficiencies were discovered. In addition there were 28 cases of squint clearly observable without the use of any test procedure.

Each of these 257 subjects was given 13 different opportunities to make unilateral sightings, such as sighting with a gun, pointing the finger, looking through a ring. These are commonly used tests of ocular dominance. The subjects were all elementary school children ranging in age from six to twelve.

The relationship between unilateral sighting preferences,

TABLE I

THE RELATIONSHIP OF EYE MUSCLE INSUFFICIENCY TO UNILATERAL SIGHTING

Sighting Eye	Frequency			
	R.E. Squint	L.E. Squint	R.E. Insuff.	L.E. Insuff.
12.5– 13.5		15		142
10.5– 11.5				8
8.5– 9.5		1		10
6.5– 7.5		1		1
4.5– 5.5			1	
2.5– 3.5			1	3
.5– 1.5			1	2
– 1.5– – .5				3
– 3.5– – 2.5				3
– 5.5– – 4.5			2	
– 7.5– – 6.5			4	
– 9.5– –10.5			3	
–11.5– –10.5			4	
–13.5– –12.5	11		39	2
Total	11	17	55	174

The interval 12.5–13.5 indicates that 12 or 13 of the sightings were made with the right eye; the interval –13.5– –12.5 indicates that 12 or 13 of the sightings were made with the left eye.

or eye dominance, and eye muscle insufficiencies is shown in Table I. Little analysis is needed to show that there is a very close relationship. There are only two outstanding exceptions in the 257 cases. It is possible that some refractive or astigmatic error of serious nature was instrumental in causing these exceptions.

In summary, the eye with the muscle insufficiency as herein defined is seldom the sighting or dominant eye. It also seems reasonable to believe we have another valuable method for determining eye dominance.

(Manuscript received November 14, 1934)

DISCUSSION

ON THE "FACTORS INFLUENCING THE LATENT TIME OF THE PATELLAR REFLEX."

In his interesting study on 'Factors influencing the latent time of the patellar reflex,'[1] W. Varnum, pointing to the shortening effect of the 'Jendrassiks Handgriff' and, in a general way, of the latent time of the reflex, states: "It seems strange that this interesting parallel to the 'Jendrassiks Handgriff' should have waited so long for its experimental investigation." But, Mr. Varnum shows, in so doing, that his knowledge of previous studies, and more particularly of those written in French, is very far from being complete. He will be able to persuade himself of it by looking into some of the works I had the opportunity of quoting in my recent study on "Interprétation de la réduction, avec l'intensité du stimulus, du temps de latence des réflexes tendineux."[2] But, in order to show that the observation of the shortening effect of the 'Jendrassiks Handgriff' has not waited, to be made, until 1934, I shall take the liberty of reproducing here some lines of a report I made to the 'Société de Biologie' in 1917 (*Comptes Rendus et Mémoires*, **80,** 6 ff), and where I showed that an increase of patellar reflectivity calls forth, for a constant intensity of stimulation, a shortening of the latent time, and more particularly of the 'Jendrassiks Handgriff,' the phenomenon being very general in its character.

« Chez un même sujet on peut aussi trouver quelques variations d'un moment à l'autre. Un facteur connu de ces variations, c'est l'intensité de l'excitation, facteur qui agit sur toutes les formes de temps de réaction.[3]

« François-Franck avait signalé le fait chez le chien dès 1888, en procédant à des excitations électriques,[4] et l'avait confirmé ultérieurement en procédant à des enregistrements cinématographiques.[5]

[1] J. Exper. Psychol., 1934, **17**, 556–573.

[2] *Arch. internat. Physiol.*, 1934, **38**, 443.

[3] Cf. H. Piéron. Recherches sur les lois de variation des temps de latence sensorielle en fonction des intensités excitatrices. *Année Psychol.*, 1914, **20**, 17–96.

[4] Recherches sur la rapidité des réactions motrices réflexes. *Mem. de la Soc. de Biol.*, 1888, p. 17–26.

[5] Application de la méthode graphophotographique à l'étude des réflexes tendineux. *Comptes-Rend. Soc. Biol.*, 1904, **57**, 9–14.

« On peut constater cette influence chez l'homme en procédant à des percussions tendineuses d'intensité variable.

Voici quelques exemples sur 5 sujets (réflexe rotulien), l'excitation faible correspond au seuil:

	A	*B*	*C*	*D*	*E*
Excitation faible.....	0 sec .053	0 sec .070	0 sec .061	0 sec .060	—
Excitation moyenne.....	0 sec .051	0 sec .053	0 sec .056	0 sec .055	0 sec .040
Excitation forte.....	0 sec .047	0 sec .048	0 sec .052	—	0 sec .037

« On obtient comme moyennes, avec les trois premiers sujets, pour des intensités croissantes: *0 sec .061*, *0 sec .053*, *0 sec .049*.

« Avec une intensité d'excitation constante, on peut encore obtenir des temps de latence variables; cette variabilité coexiste alors avec une variabilité parallèle dans l'intensité des réponses, et se manifeste surtout quand la réflectivité est pathologiquement irrégulière. Mais, chez un sujet normal, il existe un moyen classique de provoquer une augmentation de l'intensité du réflexe, c'est la manoeuvre de Jendrassik; or, on observe alors, parallèlement et je dirai même proportionnellement, à cette augmentation d'intensité, une réduction du temps de latence.

« Voici quelques exemples:

	A	*B*	*C*	*D*	*A'*
Percussion simple.....	0 sec .054	0 sec .064	0 sec .047	0 sec .046	0 sec .044
Manoeuvre de Jendrassik.	0 sec. 044	0 sec .057	0 sec .041	0 sec .044	0 sec .043

« La différence des temps qui atteint 0 sec .010 pour le premier sujet, et se réduit à 0 sec .002 chez le quatrième, décroît en même temps que l'efficacité de la manoeuvre: si, dans les deux premiers cas, l'intensité du réflexe s'était trouvée énormément accrue, elle ne l'avait été que fort peu dans les deux derniers. En particulier le premier cas est relatif au côté sain d'un hémiplégique d'origine spécifique, et le dernier au côté paralysé du même malade; or, de ce côté paralysé, les réflexes, très exagérés, n'étaient pas augmentés nettement par la manoeuvre de Jendrassik, à l'inverse du côté sain.

« Aussi peut on dire que le facteur qui agit toujours au point de vue de la variation des temps de latence chez un même individu, c'est l'intensité de l'excitation, en envisageant, non l'intensité brute, mais l'intensité efficace évaluée en multiples de la valeur correspondant au seuil d'excitabilité à un moment donné.

« Mais il y a, même à égalité d'intensité efficace, et par exemple au seuil lui-même, des différences d'un sujet à l'autre, surtout notables dans les cas pathologiques. Ce caractère individuel est alors fonction de l'excitation médullaire, l'hyperexcitabilité (avec seuil bas), s'accompagnant d'une plus grande brièveté des temps de latence.

« En somme la brièveté des temps de latence est fonction de l'excitabilité, soit absolue (différences individuelles) soit relative (variations, chez un même individu, corrélatives d'intensités efficaces plus ou moins grandes).

« Les variations des temps de latence observées au cours de la chloroformisation (Parisot) ou après section médullaire (François-Franck, Philippson), sont conformes à cette loi: C'est toujours parallèlement à l'excitabilité réflexe que varie la brièveté des temps de latence.» [6]

[6] De même, Langendorff (Beiträge zur Reflexlehre, *Arch. f. d. ges. Physiol.*, 1909, p. 517–528) a montré que, lorsque deux excitations réflexogènes se suivent à 0 sec .04 d'intervalle, chez la grenouille, le temps de latence du deuxième réflexe est diminué: l'excitabilité se trouve augmentée en effet parce que l'inertie du centre réflexe a été vaincue parbl'excitation précédente. Des faits identiques se rencontrent pour les sensations au point de vue temps de latence et intensité provoquée; il sont en particulier bien connus pour la vision depuis les recherches de A. Charpentier.

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